

## Nuclear Defence : the need for Debate

A British Government document with extended discussion of the problems of Britain's future nuclear defence is a rare landmark. A report of the House of Commons Expenditure Committee's sub-committee on Defence and External Affairs has just been published and contributes a valuable element to a debate on nuclear policy in the late 1970s and beyond—a debate which even now may peter out unless politicians, scientists and communicators continue to make sure that the public is well informed.

The central question is not difficult to understand, however complex and equivocal the answer may be. In 1962, Britain purchased from the United States four nuclear powered submarines each capable of firing 16 Polaris missiles. A Polaris missile can carry one megaton weapon or three devices of 200 kilotons each. Although the triplet can split in flight to land in a pattern, it is not possible to tailor the patterns to individual target areas. That is, present Polaris missiles are not multiple independently-targetable re-entry vehicles (MIRVs). A fleet of four submarines ensures that at least one boat is always on station. The US operates forty-one such submarines. There appears to have been general satisfaction both with the fleet's contribution to national security and its relative cheapness (about 1% of the Defence Budget goes in maintenance of the nuclear deterrent).

The question to be faced within the next year or two is whether to use the upcoming refits of these submarines to make any major modifications to their missile constituent. Many options are open at the present and the choice has to be made on the basis of three considerations—political views on Britain's nuclear role in the world, military assessments of the nature of the defence which will have to be overcome and economic judgement on cost-effectiveness and the ability of Britain to pay for improvements. The choices are as follows. The missiles could be left as they are. They could be MIRVed with British technology. They could be replaced with the larger American Poseidon missiles, probably MIRVed (10 to 14 warheads in each of the 16 missiles, each warhead being 40 to 50 kilotons). Other more exotic weapon systems are further down the line and do not need immediate attention.

The United States has refitted a majority of its boats with MIRVed Poseidon missiles but the United Kingdom should not automatically follow suit as the two countries have different defence needs. Britain cannot plan to raise its nuclear arsenal up to a level at which it could consider demolishing either the whole Russian missile launching capability or a large number of Russian cities, industries, dams and so on. So it must work on a minimum deterrence posture—that in attempting to conquer Great Britain the Soviet Union would have to weigh the probable loss of a major city. SALT has helped the British cause substantially since the restrictions on ABM sites make it likely that an attack on Moscow would at least inflict some

damage, whereas trends in ABM development would otherwise have protected Moscow for the near future from the relatively primitive assault of Polaris missiles.

The MIRVing of missiles is done for two purposes ; to overwork ABM defences (which we have just seen are not going to be as good as they might have been) and to allow several targets such as individual missile silos to be attacked in a controlled manner. Since a British nuclear force (used independently) would have to be used against a city to appear as even a minimum deterrent there thus seems little point in MIRVing the payload, whether it is Polaris or Poseidon.

The Commons committee was in an almost impossible position when trying to view these issues from the financial point of view. It is clearly not profitable to speculate too extensively on the economics of various options without knowing whether they make sense in political and military terms. Thus the committee, led by the vigorous questioning of Dr David Owen, strayed almost continuously from its brief into attempting to understand the fuller picture. This was the right thing to do in the circumstances as there is a desperate need for a public forum for the whole question.

As it was, the evidence that the committee obtained was inadequate for the purpose of making anything approaching an economic assessment of nuclear options, but excellent for understanding the options themselves. Mr François Duchêne of the International Institute for Strategic Studies presented a lucid report on technological alternatives in the next ten years, although of course he was not in a position to do more than give order of magnitude guesses of the costs involved. Mr Richard Garwin and Dr Donald Brennan submitted memoranda from the United States on technological and political questions of a British deterrent. But the person from whom the committee should have expected the most specific evidence—Lord Carrington, Defence Secretary—turned in a performance which was unsatisfactory by any standard, and the committee let it be known that they were far from pleased. Apparently the ministry's memorandum (not published) was mainly an essay on security and Anglo-US collaboration and in the words of the report "was not very informative on time scales and costs, nor on the various possible successors to the present systems". Neither did the Defence Secretary add much in his testimony. The committee observed sourly "he also rather surprisingly said that he did not know the probable costs of the various options open to the Government".

On the basis of the evidence given, the committee was right to conclude that "there is a strong case for keeping the British strategic nuclear deterrent as it is", although it lacked anything like a detailed costing of the options. There certainly should be no further scaling of the strategic ladder without an extensive debate on what is required politically and militarily, and this debate needs to



be in public. Alas, if the Ministry of Defence had their way public discussion would be frowned on because not all the facts would be available. The ministry should look across the Atlantic and appreciate the virtues of informed external criticism and the amount of money it undoubtedly saved the American taxpayer when inflated internal demands for the best ABM defence that money could buy were met with detailed arguments from obviously competent outsiders. The time is long past when defence issues, particularly those involving hundreds of millions of pounds, can be settled quietly within Whitehall by the armed services without external debate. The results of a policy of excessive secrecy are clear in this report. The committee accepts, with little question, the conclusions of Messrs Duchêne, Brennan and Garwin because it lacks any counterbalancing view from Whitehall which thinks it is all too secret to talk about.

It will be a matter of some surprise that the testimony came from two Americans and the director of an international institute. Were there no British experts capable of enlightening the committee about their home country's needs? No British Brennans and Garwins, or for that matter Wiesners, Tellers and Rathjens? If not, this must be the result of a lack of flow of top level academics into and out of defence science and engineering. Such flow would have the inconvenience for the ministry of fostering informed criticism but the virtue for the country of preventing stagnation of thinking within the ministry.

Fortunately all is not lost for the scientist who would like to understand and participate in the debate on Britain's nuclear future. A large number of the relevant facts are already widely known and published. Beyond those facts anyone with intellectual curiosity can infer rather accurately the state of the art in defence hardware and software, and may even, by naively believing that defence thinking is always first-rate, be a year or two ahead of reality in his projections. It is simply untrue to claim, as does Lord Carrington, that "unless one . . . has access to the detailed knowledge which only the Ministry of Defence can have, I do not believe one will necessarily come to the right conclusions". Scientists have no end of case-histories to confound that sort of thinking. Perhaps the Ministry of Defence knows it will get its way regardless and therefore cares little for this committee. But if the community of those who are concerned for an effective and economical defence policy takes this recent encounter as a warning to do its homework and start independent thinking, the committee will have served its purpose.

## On the Right Road

THE British Association for the Advancement of Science has put itself through an exacting series of hoops during the past few years. Only five years ago it was in deep trouble both financially and in its scientific vitality; now there are encouraging signs of life, as last week's meeting at Canterbury showed. The constitution is new and slimmer, and under Sir Eric Mensforth's guidance as treasurer, the books balance.

But what of the science? The BA has always had to look in two directions when presenting science—to the scientist to ensure fairness and accuracy and to the layman to ensure interest and comprehension. As long as the

annual meeting was the only occasion that the association was visible to most people, there was widespread feeling among both scientists and laymen that the BA was superficial. When the expenditure of public funds on the BA was negligible, there was little that could be done about this, but now that there is an annual subvention from the government (via the Royal Society) of £20,000 change is necessary.

It is thus most welcome that the BA is aiming for more visibility the year round. At a press conference held during the meeting this year's President (Sir Kingsley Dunham) and Dr Magnus Pyke, the council's chairman, promised that the BA council would offer its opinions on scientific matters in the news as fast as it is able, although whether that will be fast enough for the daily press remains to be seen. Furthermore in the past year the association has produced plans for its future activities which if they are fulfilled could see the association meeting a much felt need among British scientists.

During the next year the association is to publish a number of "green papers" on items of current concern—for example on the funding of university research and on the ethical implications of trends in modern biology—and further studies on similar matters are promised. The concern for a new role for the BA is not confined to Sir Kingsley and Dr Pyke; a number of the association's section presidents in their addresses see the BA as a forum where important matters of public interest can be studied and aired.

How is this to be done? The answer lies in the appeal that the association is launching. Companies and foundations are to be asked to sponsor particular activities of the BA, and these will include seminars and study groups on current issues. The association declares itself willing to have commercial names linked to its work. But it must be said that herein lies a danger. The reason the BA is the ideal place for matters of public moment to be aired is that the association owes allegiance only to its members and to science. If its comments on environmental or other matters are to carry any real weight they must not merely be independent, but must be seen to be so.

## 100 Years Ago



THE additions to the Zoological Society's Gardens during the past week include two Persian Sheep (*Ovis aries*), presented by Mr. W. H. Shirley; a Diamond Snake (*Morelia spilotes*) from New South Wales, presented by Mr. H. Frieland; two Robben Island Snakes (*Coronella phocaenae*), presented by the Rev. G. H. Fisk; two Chubb (*Leuciscus cephalus*) and a Barbel (*Barbus vulgaris*) presented by Mr. E. S. Wilson; two Ring-tailed Lemurs (*Lemur catta*) from Madagascar; a King Parakeet (*Aprosmictus scapulatus*) from New South Wales; a Black Cuckoo (*Eudynamis orientalis*) from India, purchased; a Weeper Capuchin (*Cebus capucinus*) and a White-throated Sajou (*C. hypoleucus*) from America, deposited.

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## OLD WORLD

## Mellanby on the Decline of Science

by our Special Correspondent

*Canterbury, August 24*

AN attack on the organization of research, every bit as sweeping as was promised in the pre-meeting publicity, was delivered by Professor Kenneth Mellanby, Director of Monks Wood Research Station, at last week's British Association meeting in Canterbury. Before the conference met Professor Mellanby promised to review the changes made since 1945 to improve research efficiency, to illustrate how they have almost all been counter-productive and to provide a solution. And he did just that.

Up to 1939, Professor Mellanby said, Britain received a good return from the money spent on research. Now the return was a poor one. The formation of research groups into rigidly organized teams, overstaffing of laboratories, the use of highly trained technicians and the appointment of the best scientists as administrative heads have all led to a decline. Since 1938, Professor Mellanby said, overall government expenditure has increased twenty times but spending on universities has risen two hundred times and civil research expenditure some four hundred times. But "I do not believe that there has been anything like a commensurate increase in scientific productivity, either in quality or quantity".

Professor Mellanby went on to attack the theory that there has been an information explosion. Many new journals are review journals and "I do not think that the production of original research publications has more than doubled". Quality has not increased either. "I see nothing to suggest anything other than fall-off in the appearance of work of exceptionally high quality in many fields of science".

Why has this decline occurred? "Success in original research depends almost entirely on the worker realizing the significance of his observations" Professor Mellanby said. "In considering research I am therefore an uncompromising elitist—I believe that success and originality depend almost entirely on the efforts of a very few gifted individuals."

Up to 1939 professors and heads of departments produced large numbers of papers, now they may do no original research for years. Departmental staffs are now five times larger and the best scientists are being sterilized by being

used as administrators. Rigidly organized teams, expensive recording equipment and the employment of highly skilled technicians are all counter-productive measures as the observations are now made by junior staff and not by the seniors who might realize the significance of what they see. Nowadays senior staff only interpret other people's observations.

The increased number of PhD students is also counter-productive, Professor Mellanby said. Few theses resulted in original publications in spite of the "originality" of work theoretically required to obtain a doctorate, and far too much time was spent supervising PhD students. Back in the 1930s, Professor Mellanby said, his supervisor, Professor Patrick Buxton, gave him less than 1% of his time.

The lengthy reorganization of research that has followed last year's report by Lord Rothschild has been planned by people who in the 1930s would have been doing their most important research and "the new chief scientists appointed by the ministries would probably be better off giving vent to their ideas at a laboratory bench".

"My main worry about the situation is that it is one more unnecessary time-consuming complication which will produce a further host of unproductive administrators. Already several previously productive scientists are spending hours, which they might have spent in their labs, wrestling with unnecessary and often bogus financial estimates."

So what is the Mellanby solution? His answer is simple. Many of the staff currently in the research councils should be placed in development authorities while original workers should be identified and supported by the research councils in separate laboratories without too much expert assistance. They should be given a three year contract and no security of tenure and allowed to get on with their work. At any time the research worker could withdraw from original research and go to a development authority laboratory where the ideas generated by the research laboratories are exploited.

"The next important change is to cut down the time spent on committee meetings. A total ban on committees for a year would probably be a good first step, though a compromise with a few important ones meeting much less frequently would be more likely to meet with agreement". Committees should also

prune themselves dramatically allowing those members with better things to do—in other words their own research—to resign first. And administrators who are scientists should be allowed back to the laboratory bench.

But while Professor Mellanby was planning to reorganize research so that the gifted few can give full time to their ideas, others were presenting the results of their efforts under the present system.

Sir George Porter, Director of the Royal Institution, described how solar energy might meet future power demands, trials of a new drug—'Arvin'—which may prevent the recurrence of thrombosis were revealed, the dangers of computer dating, of tea-drinking while pregnant and of the repeated oil pollution of estuaries were postulated, and work on rheumatism that may lead to improved treatments was outlined.

A symposium on Kent and Europe included a lecture by Dr I. B. Thompson of the University of Southampton which concluded that France had a great deal and Britain rather less to gain from the Channel Tunnel, while an industrial forum on the dangers of instability in our society due to scientific and technological advance failed to get to grips with its subject matter, and Hilary Rose traced the rise and fall of the British Society for Social Responsibility in Science.

All in all the 1,500 who made it to Canterbury had rather better fare than last year, and the association plans to hold a meeting next year in Stirling where the projected symposia on Scotland and North Sea oil should show whether the association is a viable forum for the discussion of matters of public moment.

In spite of the disappointingly small size of this year's meeting the association seemed very much alive and its plans to hold a student conference in January 1974, tied to the intention of giving both the students, and the school children who make up the British Association Young Scientists, a say in forming the section programmes in future, augur well.

Dr John Kendrew is the president of the association which now has its new constitution. A general committee of 59 members instead of 350 and a council of 22 instead of 75, aided and abetted by a full time council chairman is now in operation following the Privy Council's approval of the changes earlier this year.

ANZAAS

## Scientists Go Political

from a Correspondent

A COMPLETE and quite dramatic change in the relationships between scientists, the bureaucracy and the political world in Australia marked the recent 45th Congress of ANZAAS, the Australian and New Zealand Association for the Advancement of Science. The Congress, held in Perth from August 13-17, was the first big scientific conference since the change to a Labor Federal Government last year.

When the Whitlam Cabinet was formed in Canberra in December, several changes of significance to scientists were incorporated in the altered departmental structure, compared with the days of the Liberal/Country Party coalition which had governed Australia for the previous 23 years. These were strikingly reflected at the Congress.

Before last December, the only federal government department involved directly in scientific activity was the Department of Education and Science; its major effort was in education. The Labor Government split this in two, thus providing a separate Department of Science for the first time, although it should be added that this department, like a number of others, is controlled by a Minister with more than one portfolio (the Minister for Science is also Minister for External Territories, the latter task demanding much of his attention as Papua-New Guinea approaches self-government). New Departments of Environment and Conservation, and Urban and Regional Development were also created, largely as a result of extensive consultations between the Labor Party, when in opposition, and the scientific and academic communities.

ANZAAS is comprehensive in its coverage of academic disciplines. Ever since its first Congress in 1888, it has included non-scientific and semi-scientific subjects like history, anthropology, economics, education, sociology, architecture and town planning and psychology. Consequently, the ANZAAS Congress was an excellent testing ground for evaluating the extent to which the academic and research communities of Australia, on the one hand, and the political and bureaucratic structure, on the other, are getting on with each other under the new government.

In contrast with the Liberal/Country Party days of power, when relations between science and politics were marked by antipathetic philosophies and some mutual suspicion, the many Labor politicians who came to ANZAAS fitted easily into the scene, delivering papers in their own right, and participating freely in discussions on contentious issues. The Federal Ministers of Science, Environ-

ment and Conservation, Social Security, Immigration, Education, and Urban and Regional Development were all on the list of speakers. Several ministerial advisers and departmental heads also took part (both of these groups in Canberra now contain a significant number of scientists for the first time). Extensive, open discussion and lobbying with politicians and their staffs were features never previously observed at an Australian science conference. This was particularly timely in that the Congress preceded by a few days the handing down of the first Labor budget, in a Parliamentary session which will include many measures of importance to science and technology.

The ANZAAS organizers defined the congress theme as "Science, Development and Environment". Each afternoon was devoted to three major symposia on variations of this theme, with a considerable degree of overlap. This is illustrated by the titles of some of the symposia: "The Scientists, the Bureaucrat and Environmental Responsibility", "Limits to Growth: Population in Australia", "Resource Management and Planning", "Education and Environment", "The Implications of Nuclear Explosives", "Whither Urban Australia?", and "Ord River Ecology". It was in these symposia that most of the political content was included. The largest press, radio and television corps in ANZAAS history backed up by excellent facilities made sure these aspects were widely reported.

On the campus of the University of Western Australia the 2,900 professional delegates were joined by a record 2,300 students. Secondary school students were given leave to attend the Congress and this younger generation was active in questioning speakers, notably in the smaller sessions. Eight hundred and fifty papers were delivered in 24 different specialist sections. Although New Zealand is meant to be an equal partner in ANZAAS, there were few delegates from that country, whose capital Wellington is roughly as far from Perth as Sverdlovsk is from London. It was inevitable, then, that those scientific developments which received attention were related virtually exclusively to Australia.

### CEREALS

## Shorter Wheat

A FEW weeks ago there was great consternation that this year's wheat crop in Britain had been ruined by high winds. But fortunately the good weather and abundance of sunshine recently, combined with the fact that the wheat was nearly ready for harvesting anyway, has ensured that most of the crop was saved. It could well have been a

different story if the past two weeks had been wet and stormy.

Cereal crops can still be harvested after they have been partially flattened by winds or rain but if the crop is flattened early in the summer before the cereal is ripe then the erect crop ripens before the flattened cereal, which causes difficulties at harvesting time. Another difficulty with flattened cereal is that weeds are much more likely to grow through and smother the crop, resulting in loss of yield. But of greater importance, the laid crop will not be of the best quality.

Clearly it would be of great advantage to the farmer if the cereal was less prone to flattening by winds and rain and, in answer to this, one of the aims of plant breeders is to produce cereal which has either a stiffer straw or one which has a shorter straw—the so called dwarf cereals. The farmer has another purpose in demanding short stem cereals, and that is that nowadays farmers do not need as much straw as they did several years ago. Chemical fertilizers have now mostly replaced farmyard manure for which straw was needed.

There is little doubt that in the coming years dwarf cereals will gradually replace the longer stem varieties. But dwarf varieties of wheat, in particular, have some way to go before they can match the longer stem varieties in yield, quality and resistance to disease.

The grain of dwarf varieties of wheat at present being tested in national trials is soft milling and of a poor bread making quality. Most of the wheat used for bread making in Britain comes from overseas.

During recent years, however, more and more of the softer British wheat has been used in making bread of an acceptable quality because of improved milling and baking techniques. Even so about two-thirds of British wheat is still used for animal feed or for making biscuits.

Another difficulty with dwarf wheats is that control of tall weeds may be more difficult than it is with longer stemmed varieties. Some grass weeds in wheat fields are controlled by spraying with chemicals but it has been found that many of the dwarf varieties like some of the taller wheats are readily damaged by these chemicals. But it is possible that the early vigour of many dwarf varieties will provide an environment in which most weeds will fail to grow.

It is also possible that short stem wheats will be more susceptible to *Septoria* leaf- and glume-blotch because the shortness of the stems could lead to a humid environment conducive to the development of these diseases. It has been suggested also that spread of disease by rain splash will be easier in the short stem varieties.



## NEW WORLD

# Publishers step up Fight against Photocopying

by our Washington Correspondent

A LEGAL and legislative battle, the outcome of which will have important implications for the scientific community, is taking place in the United States over copyright. At issue is the right of libraries to provide researchers with photocopies of published material, such as scientific articles. And, according to some publishers, the survival of specialist journals is also at stake.

The matter has been brought to a head recently by two separate developments, the first of which is a court case brought by a publishing company against the National Institute of Medicine, and the second is a bill to amend the 1909 Copyright Act, which is under consideration by a Senate subcommittee. In general, the battle lines are loosely drawn between publishers on the one hand, who are concerned that extensive photocopying of journal articles will cut into subscriptions, and libraries and their clients on the other, who are anxious that harsh enforcement of the copyright laws will stifle research and cripple the interlibrary loans system by which resources are shared between libraries.

The court case was brought last year by Williams and Wilkins Co., a publisher of some 37 medical journals including the *Journal of Immunology* and *Medicine*, who charged that the National Library of Medicine and the library of the National Institutes of Health, had infringed the copyright laws by providing photocopies of journal articles to scientists and to other libraries. Their suit, the first judicial review in more than 60 years of the so-called "fair use" provision of the Copyright Act, was filed in the US Claims Court, a special court for claims against the federal government.

The publishing company won the first round on February 16, when Commissioner James F. Davies ruled that the libraries had been photocopying on such a large scale that they had indeed infringed the copyright laws. But the government has appealed the case, and it is expected eventually to end up in the US Supreme Court.

Meanwhile, on Capitol Hill, a bill introduced into the Senate by Senator John McClellan of Arkansas and under consideration by his subcommittee on Patents, Trademarks and Copyrights, is providing another battleground. Designed to bring the 1909

Copyright Act into the age of broadcasting and global communications, the bill contains a "fair use" provision which would allow individuals to make copies of copyright material for such purposes as criticism, news reporting, teaching, scholarship and research. In deciding what constitutes fair use, the bill suggests that such factors as "the purpose and character of the use, the nature of the copyrighted work, the amount and substantiality of the portion used in relation to the copyrighted work as a whole; and the effect of the use on the potential market for or value of the copyrighted work" should be taken into account. But it also contains a provision which would severely limit the right of a library to make a photocopy of copyrighted work. It is that provision which is being fought tooth and nail by educational and library organizations.

In short, the provision would allow a library to make a copy of such material only if it is satisfied that the work could not be obtained through the usual commercial channels. In other words, no library would be able to photocopy an article from a scientific journal which is still in print and available at the usual price, without paying royalties to the publisher. McClellan's subcommittee recently held two days of hearings on the bill, at which there was a considerable clash of opinion.

Representatives of library associations and educational organizations argued that the provision would place an intolerable burden on library resources and that it would hamper scientific research. They pointed out that it would involve considerable work on the part of library staff to determine whether the material was readily available, and that the necessity for paying royalties would, in the words of Stephen McCarthy, Executive Director of the Association of Research Libraries, "cost dimes to collect pennies".

But those who oppose the restrictions are particularly concerned that they would cripple the interlibrary loans system. The system often works by a student or researcher requesting a journal article from one library, which obtains a photocopy for him from a different library. Clearly, the system holds particular benefit for small libraries which cannot afford to carry vast stocks of journals.

Publishing companies are, however, greatly concerned about the growing use of photocopying in general, and the

interlibrary loans system in particular. Kenneth Keating, the new US Ambassador to Israel, who was testifying at the hearings for Harcourt Brace Jovanovich Inc., and Macmillan Inc., summed up the situation like this: "as reprographic technology progresses, interlibrary affiliates grow, and information transfer systems develop, educational and trade publishers are likely to find the economic realities of their businesses approaching prohibitiveness". And Arthur J. Rosenthal, Director of the Harvard University Press, said "the journal publisher finds himself unable to pass on higher costs to the consumer, who at some point in the scale will prefer photocopying to subscribing".

But all the publishers were adamant that they do not want to put a stop to photocopying entirely—all they want is some compensation in the form of royalties. Robert Cairns, Executive Director of the American Chemical Society suggested, for example, that publishing costs should be shared by the users, which is not the case if unrestricted photocopying is allowed. In support of his claim that publishers are suffering from the increase in photocopying, Cairns said that in the past three years, circulation of the *Journal of the American Chemical Society* has declined from 19,419 to 16,139, and that in the first six months of this year, it has dropped even further, to 14,726.

If the publishers' arguments eventually persuade Congress to pass the copyright bill, how could a system be devised for collecting royalties which would not cost more than it brought in? One answer may lie in the bill itself, which would set up a National Commission on New Technological Uses of Copyrighted Works to study such matters. But the Williams and Wilkins Co. believes it may have a solution to the problem. The company has proposed charging a higher price to libraries than to individuals, and the extra payment would entitle the library to provide single photocopies of articles to individuals. Such an arrangement would not, however, allow the libraries to make photocopies for other libraries in the interlibrary loans system, but Mrs A. Albrecht, an official of Williams and Wilkins, said last week that the company is now considering a three tier price structure for its journals—those libraries which wish to provide copies to other libraries would pay an even higher price.

There is one argument against the restriction on photocopying by libraries which may prove to be particularly telling, however. It was pointed out that it would prevent members of Congress and their staff from receiving photocopied material from the Library of Congress and the Congressional Research Service, unless they would be willing to pay the royalties.

#### PHARMACEUTICALS

### Librium and Valium

by our Washington Correspondent

It has been a bad year for the giant Swiss pharmaceutical firm, Hoffmann-La Roche, and its two biggest money spinners, Librium and Valium. Under fire from authorities in Britain for allegedly making vast profits by overcharging the National Health Service for the two tranquillizers, and under investigation in Brussels for charging widely differing prices for Librium and Valium in different European countries, the firm received a new setback from the US government recently. The Drug Enforcement Administration, the new Federal agency concerned with drug abuse law enforcement, proposed that the two drugs should be placed under tighter control because of their potential for abuse.

The action ends a seven-year legal battle between the government and Hoffmann-La Roche over the need to regulate more tightly the prescribing and manufacture of the two tranquillizers. In short, Mr John Bartels, the Acting Administrator of the Drug Enforcement Administration, has urged that Librium and Valium be placed on Schedule IV of the Controlled Substances Act, a move whose chief effect will be to limit to five the number of times a prescription for either drug can be refilled; at present, doctors can write prescriptions for the drugs which can be refilled any number of times. The proposed regulations can hardly be described as burdensome, however, and their chief result is likely to be to make doctors more conservative in their prescribing habits and the public more cautious in taking the drugs. Some such education is clearly needed.

Even by American standards Librium and Valium are swallowed in staggering amounts in the United States. According to the Drug Enforcement Administration, 12 tons of Valium and 10 tons of Librium were prescribed last year, which is sufficient for about 20 capsules for every man, woman and child in the country. The two tranquillizers are reckoned, in fact, to be the most heavily prescribed drugs in the United States.

As for their abuse potential, however, officials in the DEA admit that the drugs do not pose as great a problem as barbi-

turates and amphetamines, for example, but the National Commission on Marihuana and Drug Abuse reported earlier this year that about 6% of US adults and 3% of youth have used minor tranquillizers for self-defined purposes. The drugs have also been reported to cause physical and/or psychological dependence, resulting in the withdrawal symptoms of insomnia, nausea, agitation, twitching, sweating and convulsions after prolonged use of high doses.

The proposal to bring Librium and Valium under the Controlled Substances Act is now being studied by the Food and Drug Administration, but no opposition is expected. Hoffmann-La Roche had fought the proposal in the courts, but has now agreed to withdraw its legal action.

#### EDUCATION

### Graduate Students Fewer

by our Washington Correspondent

THE number of students taking graduate courses in science and engineering at US universities declined last year for the third year in succession. According to figures published by the National Science Foundation, keeper of the federal government's statistics on science and technology, first year enrolments in graduate science departments dropped by almost 3% from 1971 to 1972. Every branch of science except for the life sciences reflected the drop, with engineering showing the largest decline, 5%, and physics not far behind with a reduction of 4% in total enrolments. In contrast, the number of students enrolled in graduate courses in the life sciences remained almost static.

The decline can be explained chiefly by large cutbacks in federal support for graduate students; in particular, graduate support provided by the National Science Foundation and the National Institutes of Health has been chopped in the past few years. The figures show, for example, that the total number of students directly supported by the federal government dropped by 10% between 1971 and 1972, with the life sciences again showing the smallest decline, 6%, and mathematical sciences the most affected, declining by 20%.

But, while graduate student enrolments in the sciences have been decreasing, postdoctoral appointees have been on the upswing—their numbers increased by 8.6% from 1971 to 1972. That trend, the NSF suggests, is "an indication that the tightening of the job market for PhDs in industry, higher education and government resulted in the absorption of more postdoctorals into graduate science education programs than ever before". The NSF's figures are based on a survey carried out

in the autumn of 1972 of all US colleges and medical schools granting PhD degrees.

#### IMMIGRATION

### Brain Drain Slows

by our Washington Correspondent

EARLY in 1971, the federal government tightened up its immigration regulations, by stipulating that aliens could work in the United States only if they have a job offer for which domestic workers are not available and if their employment does not affect domestic wages and working conditions. The effect of the regulations on the brain drain has been significant. According to figures released last week by the National Science Foundation, the number of immigrant scientists and engineers admitted to the United States in fiscal year 1972 dropped by 14% compared with 1971. In absolute terms, 11,300 immigrant scientists and engineers were admitted to the United States in 1972, 13,100 in 1971, and 13,300 in 1970—the peak year. The most marked decline has been in the field of engineering, and the figures also reveal that 70% of all scientists and engineers who entered the country last year were born in Asia.

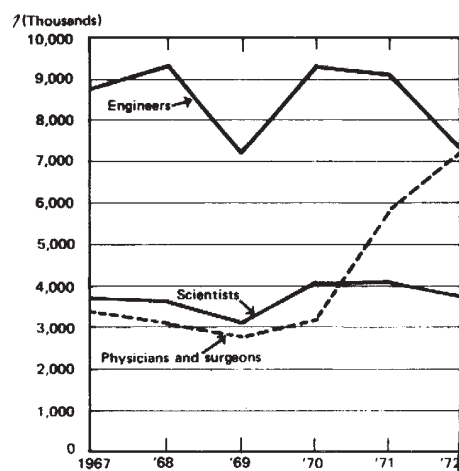


Fig. 1. Immigrant scientists, engineers, physicians, and surgeons, by broad occupational group, FY, 1967-72.\*

\*Source: National Science Foundation.

In contrast, the number of immigrant physicians and surgeons admitted to the US in 1972 jumped to 7,100, from 5,700 the previous year. In the past, no more than 3,300 physicians and surgeons entered the US in any one year. The reason for the rapid increase in numbers is that there is a shortage of personnel in the domestic medical profession, and so immigration restrictions are not so stringent. It goes without saying, however, that many of the physicians and surgeons who come to practice in the US leave countries where the shortage is even greater.



## NEWS AND VIEWS

## Preponderance of Theories of Gravity

THE only precise tests of the applicability of the theory of general relativity to the actual physical world are those long ago proposed by Einstein, and certain other tests proposed more recently that are closely related to Einstein's. All these tests are confined to laboratory experiments or astronomical observations within the nearer parts of the Solar System. They test the theory only to the so-called post-Newtonian approximation, roughly speaking to terms in  $1/c^2$ , where  $c$  is the speed of light. No observation has been found to contradict the theory to this approximation. There is always a possibility, however, that new non-relativistic phenomena will be discovered (such as a fast rotation of a central part of the Sun that could lead to an effect upon the perihelion motion of the planets, about which conjectures have from time to time been made) that may necessitate a revision of the effects to be compared with the predictions of relativity theory. In that case it might have to be recognized that general relativity would not agree with observation in the post-Newtonian approximation. A well-known modification of general relativity introduced by C. H. Brans and R. H. Dicke for a somewhat different reason does give a different post-Newtonian approximation and it, as an example, might some day be found to agree with observation better than general relativity.

As regards more general tests, general relativity apparently gives a qualitatively satisfactory account of gravitational collapse and the phenomenon of black holes, but no quantitative tests have yet proved possible. It has led to a highly developed treatment of gravitational waves, but there is no general agreement about the significance of such observational tests as have so far been attempted. Also general relativity may be said to give a qualitatively acceptable treatment of cosmological models, but the observational situation is complicated and obscure. General relativity may be regarded chiefly as a theory of the gravitational field. Attempts to extend it to include electromagnetism and other fields have met with little success in principle, and none at all in predicting new observational phenomena.

Such, in crude outline, seems to be the current state of the comparison of observation and theory in general relativity. It is disappointing. Probably few physicists would seriously doubt that general relativity provides the best theory of gravitation we possess. But probably also all physicists expect that the possibilities of measuring times and distances with the astonishing precision now attainable, the possibility of studying the effects of the so-called curvature of space out to the distances of the most remote quasars, the possibility provided by neutron stars and pulsars of studying the effects of gravitational fields about a million times more intense than any known before, as well as other new techniques and phenomena, will soon transform the situation as regards observational tests.

The recent publication of an article on certain aspects of the subject, under a mischievously provocative title (Wei-Tou Ni, "A New Theory of Gravity", *Phys. Rev.*, **D7**, 2880; 1973), is a reminder of one particular group of relativists who have been keeping the situation under

sophisticated professional review. This group at California Institute of Technology is led by Professor Kip S. Thorne. During the past few years, recent members of the group, chiefly C. M. Will, K. Nordtvedt and Wei-Tou Ni, have produced a substantial series of articles in the *Astrophysical Journal*, and elsewhere, in which they exhaustively classify theories of gravitation in a well-defined category, to which general relativity and the Brans-Dicke theory belong, and systematically analyse the possibilities of observational test. Evidently they hoped it might emerge that existing tests might already rule out theories in the category other than general relativity itself (or any theory that reduces to it for a suitable choice of parameters). They seem further to have conjectured that general relativity might be unique in the category at the level of the post-Newtonian approximation.

The "new theory" is simply a counter-example that invalidates these conjectures. It is a theory within the category that is shown to agree with all experiments to date, that for suitable values of its adjustable parameters has exactly the same post-Newtonian approximation as general relativity, and that nevertheless never reduces to general relativity in full. In the academic context, it is a considerable technical feat to have produced such an example. Presumably the chief lesson we are intended to draw at this stage is that existing experimental tests are less conclusive than they are commonly supposed to be.

The new theory is much more elaborate than general relativity; it involves four auxiliary gravitational fields (one tensor, one vector and two scalar) from which the physical metric tensor is generated by a complicated process, and it is found to involve four arbitrary parameters. As Ni remarks, there are important features of the theory that he has not yet studied, notably its ability or otherwise to produce cosmological models.

General relativity is thus very obviously distinguished from the new theory—and all others in the category—by its relative simplicity. Pure general relativity (without the cosmic constant) contains no adjustable, or "universal", parameter. (Speed of light and the gravitation constant  $G$  are no more than conversion factors which appear only if units of measurement are prescribed in advance.) Thus, general relativity in this form seems to tell us about the way in which we can describe our experience of the physical world. In so far as the distinction is significant, it does not tell us about what the world is made of, for this would presumably require the intervention of physical constants. The most significant thing about the gravitation field seems to be that it makes its appearance at this very rudimentary stage of physics. Gravitation seems not to depend upon any particular properties of matter, for these must require physical constants to specify them. If this is truly significant, then the new theory of gravity that does involve adjustable parameters would not be expected to give fresh insight into the nature of gravity itself so much as telling us how other fields might be affected by gravitation. It is much to be hoped that the classification of theories will be interpreted from this standpoint.

W. H. McC.



# Meteor Spectra and Meteoroid Composition

VISUAL meteors, or to give them their colloquial name, shooting stars, are produced when an interplanetary dust particle of mass greater than 0.1 g collides with the Earth's atmosphere and burns up. These particles are in the main porous, crumbly objects composed of loosely conglomerate sponge-like material and have geocentric velocities of about  $130,000 \text{ km h}^{-1}$ . The surface temperature of the meteor soon reaches boiling point as it collides with and adsorbs air molecules, and in this state the impacting meteor is eventually reduced to its constituent atoms by sputtering and ablation mechanisms. The available kinetic energy is dissipated as an atomic and molecular excitation spectrum, as continuous thermal radiation, as a dense ( $>10^{15} \text{ m}^{-3}$ ) column of electrons along the meteor path and finally (and principally) as heat. Less than 0.1% of the energy appears as visual luminosity.

One of the more pressing problems of meteor science is the detailed analysis of the composition of these dust particles. Obviously they cannot be collected by sounding rockets because the relative velocity is so large that they are completely destroyed on impact with the collecting surface. Only two paths remain open, one being the spectroscopic study of the resulting visual meteor, the other being the mass spectrometric analysis of the atmosphere in the meteor region (heights between 75 and 105 km) using rocket-borne spectrometers.

The typical meteor spectrum is one of low excitation, with the ionization potentials of the ions lying in the range 1.9 to 13.9 volts—this being equivalent to a temperature range from roughly 1,680 to 4,800 K. The upper state population is of the order of  $10^{15}$  to  $10^{17}$ . The following elements are observed in the spectra of the meteor head: neutral atoms FeI, MgI, NaI, CaI, MnI, CrI, AlI, NiI, HI, OI, NI; ionized atoms CaII, MgII, SiII, FeII, NII, OII. Furthermore, CoI, SrI, SrII, BaI, BaII, SiI are sometimes identified but their presence is doubtful. The meteor spectrum changes with the velocity of the particle and the luminosity of the meteor, the CaII lines H and K being the main feature for fast meteors and NaI, MgI and FeI being dominant in the spectra of slow meteors. The evanescent train for fast meteors shows a much lower excitation level than the meteor head and in general there is an absence of ionized lines. These trains (or wakes) often contain molecular lines of, for example,  $\text{N}_2$ , FeO, MgO, CaO, CN,  $\text{C}_2$ , CO and  $\text{CO}^+$ .

By applying the emission curve-of-growth method to meteor spectra of high dispersion the spectra can be quantitatively analysed. This has been done by Cepelch (Bull. astr. Inst. Czech., 17, 195; 1966) for two spectra from slow meteors and the computed elemental abundances are approximately those of an average stony meteorite. Problems arise with this method because the excitation temperature, an important parameter in the calculation, varies as a function of the length of the luminous volume. It is thus very difficult to construct the emission curve of growth for optically thin lines.

The problem of determining the element abundance of meteors from their spectra is re-examined by Harvey (NASA, Langley Research Center) in the *Journal of Geophysical Research* (78, 3913; 1973). Harvey tackles the question of self absorption in the meteor plasma in two ways; first, by comparing the spectral photometry used in the data reduction with spectrograms which indicate

optimum self absorption and, second, by comparing the observed radiant energy with that expected from an optically thick plasma. He concludes (contrary to the finding of Cepelch (IAU Colloquium, 13, NASA SP 319; 1973)) that there is negligible self absorption in most optical meteor trains. Ionization in the train, however, does affect the optical radiation because it leads to neutral atom depletion and it must therefore be considered when calculating the elemental abundances. The effect is significant for the lower ionization potential elements (Na, Ca) but negligible for elements such as Fe, Ni, Si and Mg. It is interesting to note that neutral atoms are depleted not only by ionization but also by the incomplete dissociation of stable molecules.

Harvey has analysed the spectra of five meteors in detail and the results indicate that the incident meteoroids have elemental abundances similar to those of carbonaceous chondrites, the uncertainties in the abundance determinations depending directly on the ionization and dissociation corrections. The analysis was checked by using a sample of the type 3 carbonaceous chondrite Allende, a stony meteor which fell in Mexico in 1969. This produced a comparison meteorite spectrum of known chemical composition. Using a dc arc as an excitation source Harvey found that the meteorite spectrum was very similar to that of Taurid and Geminid meteor spectra previously obtained by him. Another heartening conclusion was that the elemental abundances of Allende obtained from the spectra were in good agreement with those obtained chemically. Harvey found that of the meteor spectra analysed most indicate that the incident meteoroids have compositions similar to carbonaceous chondrites.

Meteoroids are the solid dust particles of cometary nuclei which get pushed away from the nucleus by gas pressure produced by the sublimation of the volatile cometary ices. Harvey's work therefore supports the generally accepted conclusion that cometary dust is primaevial, being made up of the primitive solid essence of the condensing nebula that formed the Solar System. Also about 99% of cometary dust and meteoroid particles are found to have the same composition, which is similar to the elemental composition of carbonaceous chondrites.

Thus the considerable variations found in the densities of meteoroids in different meteor streams and also in the sporadic background (for example, 2% of the sporadic meteoroids have been found to have an original density of  $6.6 \text{ g cm}^{-3}$ , 4%  $3.4 \text{ g cm}^{-3}$ , 10%  $1.5 \text{ g cm}^{-3}$ , 46%  $0.4 \text{ g cm}^{-3}$  and 38%  $0.05 \text{ g cm}^{-3}$ ) must be due to physical and not chemical causes.

D. W. H.

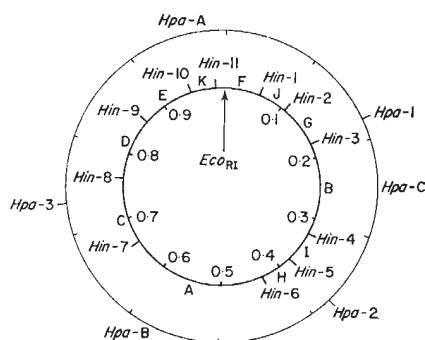
## Dissecting SV40 DNA

By comparison with the sophistication of bacteriophage genetics, the genetics of eukaryotic viruses is still a very green field. It is rapidly maturing, however, with recent studies on SV40 and polyoma virus, spurred by the hope that the genetics of these transforming viruses may elucidate the mechanism of neoplastic transformation.

Fortunately, the size of the SV40 genome ( $3 \times 10^6$ ) makes molecular dissection manageable, although smallness is a disadvantage for other reasons. It is the attribute of

small size and the availability of certain enzymes which allow analysis by specific digestion followed by isolation of the resulting small number of fragments—a procedure which would be daunting with large molecules. Specific fragments can be ordered, and so a form of molecular map can be made, regardless of whether anything is known about the nature and extent of individual genes. Provided that the fragments produced in the first place are of a size suitable for specific molecular hybridization and that RNA products of the genome in question are available, then information will be obtainable about individual genes.

Two recent papers together go far towards complete functional analysis of the SV40 genome. First, Danna, Sack and Nathans (*J. molec. Biol.*, **78**, 363; 1973) report their construction of a complete cleavage map by a meticulous and exhaustive analysis of the fragments produced by treatment with bacterial restriction enzymes. In the following paper, Khoury, Martin, Lee, Danna and Nathans (*ibid.*, 377) use the SV40 fragments to analyse the origin of the viral RNA molecules transcribed during lytic infection of African green monkey kidney cells (BSC-1).



Cleavage map of the SV40 genome.

The method used to obtain a cleavage map was chosen because the  $R_I$  restriction endonuclease of *Escherichia coli* ( $Eco_{RI}$ ) breaks SV40 DNA at one site only, whereas the restriction enzymes from *Haemophilus influenzae* and *H. parainfluenzae* break it at eleven and three sites respectively. The resulting fragments can be separated from each other on polyacrylamide gel slabs—so providing, simultaneously, information about their molecular weight and therefore about the proportion of the total genome which they represent.

In a manner analogous to the sequencing of a protein molecule,

Danna *et al.* obtained a series of overlapping fragments, so as to deduce the order of their limit products, and emerged with a map (see figure). Map distances are in relation to the  $Eco_{RI}$  cleavage site. The existence of this map means that specific pieces of SV40 DNA can be used as probes to select those RNA molecules which are complementary to them within a given population of cell RNA. Having selected viral RNA molecules, it is a comparatively short step to deciding what is the relation between the restriction fragments and individual genes.

Khoury *et al.* are concerned with the achievement of both these aims. Not only have they discovered which and when sections of the SV40 genome are transcribed during lytic infection but, also, they have determined the position in the cleavage map of the “early” and “late” template regions, and the direction of their transcription. SV40 DNA molecules labelled with  $^{32}P$  were cleaved by *H. influenzae* restriction enzyme; complementary RNA molecules were selected from “early” or “late” total RNA by hybridization; and the resulting hybrids were isolated by hydroxyapatite chromatography.

The results confirm that early transcription is from the minus strand, and in addition show that it involves four of the eleven restriction fragments (A, B, H and I). Late transcription is from the plus strand and involves fragments C, D, E, K, F, J and G, parts of A and B, and probably also of H and I. Presumably, fragments A, B, H and I contain sections of different genes, although the authors cannot say precisely which regions of them are transcribed from the plus and which from the minus strand. As to the direction of transcription, establishment of the 3' to 5' orientation of DNA strands within the cleavage map is sufficient. Khoury *et al.* show that early transcription is counter-clockwise from A to B, and late transcription is clockwise from A to B. Predictions can, furthermore, be made about the position of initiation and termination sites. A similar analysis of SV40 transcription in transformed cells is under way.

Something is already known about the function of the RNA products coded for by SV40 DNA; for example, the region of the genome which is transcribed early in lytic infection is required for expression of U and T antigens, and of the SV40

tumour specific transplantation antigen. It cannot now be long before the limits of these structural genes are defined, or, indeed, before even their nucleotide sequences are known. The information that this will provide may be of value far beyond the confines of one small DNA virus.

From a Correspondent

## VITAMIN C

### For and Against

from a Correspondent

A CONFERENCE on vitamin C and the common cold organized at Stanford University on August 6 and 7 on behalf of the Departments of Chemistry and Chemical Engineering by Professor Linus Pauling (Stanford University) provided a timely review of the current controversy on the therapeutic value of high intake of ascorbic acid and some new data. The opposition of the medical profession to the use of mega quantities of ascorbic acid—in contrast with the much smaller amounts acknowledged as necessary for prevention of scurvy—is well known. Evidence based upon double-blind trials was presented by several speakers to show that high intake of the vitamin, although not necessarily decreasing the numbers of colds, favours decrease of their severity.

Professor A. Szent-Györgi (Marine Biological Laboratory, Woods Hole) traced the history of his discovery and isolation of L-ascorbic acid—for which he was awarded a Nobel prize in 1937—and accused the medical profession of being blind to the results of double-blind trials. Dr I. Stone (University of California) outlined his original idea of hypoascorbemia in terms of a gene deficiency, discussed in more detail in his recent book, on *Vitamin C and Disease*. Professor Pauling marshalled numerous cases favouring the concept that a dosage of several grams of vitamin C rather than the accepted 100 mg dosage is required each day.

Research on the metabolism of vitamin C was discussed by Dr J. J. Burns (Hoffmann-La Roche, Nutley); man metabolizes ascorbic acid at a considerably slower rate than the guinea-pig—with respective half lives of 15 days and 3 days—which may explain why guinea-pigs develop scurvy more rapidly. Certain chemicals including barbiturates and 3-4 benzpyrene which stimulate ascorbic acid biosynthesis in the rat also inhibit markedly the activity of oxidative enzymes in liver microsomes which detoxify the drugs. The vitamin also interferes with the reaction of nitrites and secondary amines to form nitroso derivatives, which may be carcinogenic in certain animals. Dr L. Spero (University of Toronto) outlined



several of the problems of research into the pharmacological effects of high doses of ascorbic acid and reviewed methods for assaying total ascorbic acid and dehydroascorbic acid content.

The prophylactic uses of doses in excess of 1 g daily of vitamin C were considered in detail by Dr J. Coulehan (Lower Greasewood Indian Health Center, Arizona), Dr R. Colby (Stockton State College, New Jersey), Dr T. Chalmers (National Institutes of Health, Bethesda) and Dr T. W. Anderson (University of Toronto). Their experiences differed somewhat because considerable individual variations were recorded. Although in some cases a trend towards freedom from attack was noted, a more common effect was a reduction in the severity of colds. Some concern was expressed about the proportions of interchanges of tablets between patients and about the extents of reliability on the types of placebo tablets. The problem of bias in the results was also considered by Professor D. M. Hegsted (Harvard University School of Public Health, Boston), and Professor R. Hodges (University of California, Davis) considered that the available evidence cannot be taken as unequivocal in favouring the intake of mega quantities of the vitamin. Disquiet was expressed by more than one speaker, in particular Professors Pauling and Szent-Györgi, about reports that tablets for large doses of vitamin C may be banned in the United States.

The effects of vitamin C on viruses were considered by Professor T. Merigan (Stanford University School of Medicine); some viruses are susceptible within a short time to the presence of ascorbic acid, compared with HCl, whereas others are hardly affected. Dr S. Lewin (North East London Polytechnic, London) suggested physico-chemical explanations for some of the results obtained with vitamin C, proposing that the interfacial tension of water may be reduced (which may account for the influence of ascorbic acid on viral organization) and that ascorbic acid may break down in aqueous solution. Some of the negative results obtained in ascorbic acid trials might be due to the proportion of ascorbate salt used, leading to inactivation *in vivo*. The difference in pH between the stomach and duodenum may be important in this reaction.

Concluding the conference, Professor Pauling re-evaluated available data, including those presented in his book on *Vitamin C and the Common Cold*, and considered them to be in favour of the thesis that mega quantities of vitamin C are beneficial. The forum provided at the conference for both proponents and opponents of the use of mega quantities of the vitamin prompted a useful evaluation of available data.

## POPULATION ECOLOGY

### Evolutionary Stability

from our Animal Ecology Correspondent

FOLLOWING hot on the heels of Maynard Smith and Slatkin's paper on stability in predator-prey interactions in vertebrate animals (*Ecology*, **54**, 384; 1973), comes an equally pleasing one from van den Ende on the ecological and evolutionary relations at work within continuous culture systems of invertebrate carnivores and their prey (*Science*, **181**, 526; 1973). When Gause carried out his now classic experiments on the dynamics of mixed populations of *Paramecium* and *Didinium*, he reinoculated his cultures every 3 or 4 days to prevent elimination of one or other of the species. The result was a series of quite violent oscillations. It has been estimated that about 65% of the *Paramecium* would have had to be saved from predation for stabilization of the system.

Van den Ende established cultures in glass culture dishes of the protozoan *Tetrahymena pyriformis* which feeds on the bacterium *Klebsiella aerogenes*. The physical environment was held constant, so ensuring that fluctuations in population density were only the result of interaction between the two species. During the 1,000 hours following the introduction of the predators among their prey, five oscillations in bacterial density were observed; each was shadowed by an oscillation in protozoa density. Towards the end of the time period the frequency and amplitude of the oscillations became less and it seemed that some degree of balance had been achieved. The fascinating aspect of this work is precisely how this equilibrium came about.

According to Gause's "Principle of Competitive Exclusion", no two species can share the same ecological niche. Therefore, unless two niches had been available in van den Ende's seemingly homogenous environment, one species should have become extinct, as one invariably did in Gause's experiments. It seemed, however, that the walls of the glass culture vessels provided sufficient heterogeneity to allow the co-existence of the two species.

A rapid increase in the number of bacteria between 150 and 200 h following introduction of the predators was marked by the disappearance of polysaccharide capsules around the bacteria and the appearance of non-capsulate colonies. Non-capsulate colonies stick to glass very well and require a force of  $10^{-8}$  dynes (when the culture is stirred at 300 revolutions per min) to dislodge. *Tetrahymena* is a ciliary feeder and is unable to increase its feeding currents to that extent. Thus once on the wall of the vessel a prey item is relatively inaccessible to the predator, and it can be said to have occupied a different ecological niche from its fellows still at large in the liquid phase. When bacteria from the walls of the experimental culture were placed in control vessels they showed a marked preference for growth on solid surfaces and in large non-capsulate aggregates. Thus it seemed that a genetic change of fundamental importance to the bacterial population had taken place as a direct result of its exposure to predation.

Predatory organisms are under constant evolutionary pressure to become more skilful at prey catching, and prey are under a similar pressure to become more evasive. Natural selection, in allowing a certain number to be eaten, thereby maintaining the system, favours

### Lepore and Anti-Lepore Again

STUDIES of the two unusual human haemoglobin variants, Lepore and anti-Lepore, continue to provide information on the details of protein biosynthesis. It will be recalled that the non- $\alpha$  chains of Lepore haemoglobins are  $\delta\beta$  fusion chains, formed as a consequence of homologous but unequal crossing-over of the  $\delta$  and  $\beta$ -chain genes. In Lepore haemoglobins the N-terminal segment is derived from the  $\delta$  chain, whereas in the anti-Lepore variants the N-terminal segment is part of the  $\beta$ -chain sequence.

In *Nature New Biology* next Wednesday (September 5) Roberts, Clegg, Weatherall and Ohta report that the  $\beta\delta$  chain of the anti-Lepore variant Hb-Miyada, like the  $\delta$  chain of Hb-A<sub>2</sub> and the  $\delta\beta$  chain of Hb-Lepore, is not synthesized appreciably during the reticulocyte stage of red cell maturation.

Because the  $\delta$  and the  $\delta\beta$  chains have the same amino acid sequence in their N-terminal regions, their mRNAs presumably have the same 5'-nucleotide sequence. This, however, is not present in the mRNA coding for the anti-Lepore  $\beta\delta$  chain, so that the common pattern of early cessation of synthesis is not determined solely by some feature of the base sequence at the 5' end of the  $\delta$ -chain gene.

There is evidence for untranslated regions in globin mRNAs. Roberts *et al.* suggest that these regions may be involved in the stabilization of mRNA, and that they are absent from  $\delta$ -chain mRNA. If this is the case, then the loss of coding for stabilizing regions from the  $\beta$ -chain gene during fusion to form Lepore ( $\delta\beta$ ) and anti-Lepore ( $\beta\delta$ ) genes would account for the relative instability of their respective mRNAs.

those genotypes that escape capture. The concept of genetic feedback regulating population number and bringing about stability in interactions has been extended and championed by Pimintel (*Science*, **159**, 1432; 1968). What happens is that high predator density creates strong selective pressures on the prey. This selection alters the genetic complexion of the prey to make it more resistant to attack. Such resistance feeds back negatively to limit the feeding pressure of the predator and so stability gradually results. Van den Ende's study provides a neat insight into how this process actually works.

## NUCLEAR PHYSICS

### New Giant Resonances

from a Correspondent

It has been known for a long time that a giant electric dipole resonance can be excited in all nuclei by photonuclear experiments of the ( $\gamma, p$ ) and ( $\gamma, n$ ) type. This resonance has an excitation energy of order 70 to 80  $A^{-1/3}$  MeV (where  $A$  is the mass number of the nucleus) and a width of a few million electron volts. It is visualized as the protons and neutrons oscillating in bulk against one another. There have been speculations that other giant resonances with other multipolarities might exist; for example, quadrupole motions in which the nucleon distributions oscillate between prolate and oblate shapes with quadrupole deformations. Two types of each might exist, the isoscalar, with isotopic spin quantum number  $T=0$ , in which the neutrons and protons move together in phase and the isovector ( $T=1$ ) in which they move out of phase. The latter type is expected to have a higher vibrational frequency and hence a higher energy. In order to qualify as "giants", the excitation strengths of these resonances must exhaust a large fraction of certain sum rule limits.

An exciting development recently has been the experimental discovery of a giant isoscalar quadrupole resonance in all the nuclei in which it was looked for (and, in some cases, possibly an isovector one also). One reason the isoscalar one had escaped detection before is that its excitation energy is close to that of the giant dipole (about 2 MeV lower). Furthermore, because its excitation requires electron quadrupole (E2) radiation rather than electron dipole (E1), it is difficult to observe in photonuclear work.

The experimental studies of the past few years have been pursued in two parallel and initially independent ways, namely, inelastic electron scattering and inelastic hadron scattering. The electron measurements have been made princi-

pally at Darmstadt (Buskirk *et al.*, *Phys. Lett.*, **42B**, 194; 1972) and at Tohoku University (Nagao and Torizuka, *Phys. Rev. Lett.*, **30**, 1068; 1973). About twelve nuclei have now been studied, ranging from  $^{40}\text{Ca}$  to  $^{208}\text{Pb}$ . In each case the spectra of inelastic electrons show a peak for excitation of the giant dipole and just below it another peak whose angular distribution is compatible with E2 excitation. The strength of the lower resonance exhausts most of the quadrupole sum rule limit. Also seen in some cases are indications of another peak at roughly twice the excitation ( $\sim 120 A^{-1/3}$  MeV), also compatible with E2, and it is speculated that this is an isovector resonance.

This new resonance was first excited by strong interactions in proton inelastic scattering. It was first discovered at Oak Ridge National Laboratory in the spectra of inelastically scattered particles obtained from bombardment of the nuclei by 60 MeV protons; the peak due to excitation of the giant dipole resonance was, of course, also observed (Lewis, Bertrand and Horen, *Phys. Rev.*, **C8**, 398; 1973). Later it was realized that it had been seen 15 years earlier in the spectra from bombardment by 180 MeV protons! At that time the observed peaks had been identified solely with the giant dipole resonance whose position was not known so precisely at that time. The excitation strengths observed are in approximate agreement with the corresponding electron measurements. Meanwhile the new resonance has also been seen in many nuclei by the inelastic scattering of  $^3\text{He}$  ions and alpha particles.

The angular distributions measured in the electron scattering experiments could not distinguish between E2 and E0 transitions; further the observed strengths were compatible with the sum rule limits for either interpretation. (The E0 would be visualized as a breathing mode or radial oscillation in density.) These two alternatives would not be decisively distinguished by these early proton measurements although the later measurements favour the E2 choice. The  $^3\text{He}$  and alpha scattering measurements, however, strongly indicate that the resonance has a quadrupole character. The theoretical predictions for the cross-sections for monopole excitations which exhaust the sum rule are an order of magnitude smaller than those observed, whereas the predictions for the quadrupole are in agreement with experiment. These conclusions have been confirmed by recent studies using polarized protons. The consequent left-right asymmetries are predicted to be of opposite signs for E0 and E2 and the measurements are in agreement with E2.

The  $^3\text{He}$  and alpha experiments also provide evidence for the isoscalar nature of the resonance. The alpha particle,

being isoscalar itself, would only weakly excite an isovector state, whereas the  $^3\text{He}$  ion suffers no such inhibition. The two ions, however, excite the resonance with comparable strengths.

Naturally the discovery of this isoscalar quadrupole resonance has aroused new interest in finding other such phenomena. The possibility of an isovector quadrupole state at a much higher excitation energy has already been mentioned, and there have been some indications of this from photonuclear experiments also. Electron scattering has also shown evidence for giant magnetic dipole excitations at lower energies. Preliminary work suggests that there may be fairly strong E3 excitations present as well. Of particular theoretical interest (because it relates to the compressibility of nuclear matter) would be the discovery of a giant isoscalar monopole or E0 state. A major difficulty that the experimenters have to face is that the spectra from reactions corresponding to these high excitations show large "background" components on which these giant resonances are riding; in most cases they are only giant in the sense that they take up a large part of the corresponding sum rule limits, not that their cross-sections are giant!

Clearly the existence or non-existence of such giant resonances must have implications for other nuclear phenomena, and a number of these have been explored. Such states may be virtually excited during other processes; only, however, the effective charges needed for the description of electromagnetic transitions between low excited states are mentioned.

## POLYPEPTIDES

### Corrigendum

from our Molecular Biology Correspondent

In writing about conformation of synthetic polypeptides recently (*Nature*, **244**, 254; 1973), I unaccountably mis-stated utterly one of the results of Rao and Miller (*Biopolymers*, **12**, 835; 1973). This concerns the optical effect of incomplete removal of protecting groups. I should have said that this leads to negative ellipticity, where the fully deprotected polymer has a positive Cotton effect, not the other way round. Also, in the second paragraph, "dilute salt solutions" should have read "diverse salt solutions". Apologies to those concerned and those confused.



## RELATIVITY

## Signature of a Black Hole

by our Cosmology Correspondent

IN spite of all the fuss about X-ray stars in binary systems and the difficulty of explaining the presence of these sources by any simple astrophysical model, the case that these systems contain black holes is still not proven. Certainly it is true that in some cases, at least, there is good evidence that the underlying object associated with the X-ray emission is both dark, and thus presumably too cool for nuclear burning to be holding it up against gravitational pressure, and also that the object is more massive than the limit above which a cool, non-rotating object will collapse into a singularity without such support. But recently two groups have independently pointed out that the compact X-ray sources might be associated with differentially rotating degenerate dwarfs (Brecher and Morrison, *Astrophys. J. Lett.*, **180**, L107; 1973; and Lamb and Van Horn, *Astrophys. J.*, **183**, 959; 1973).

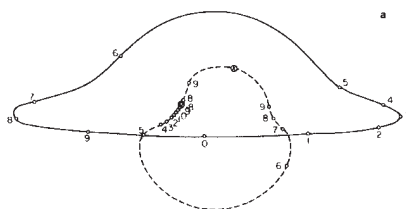
Lamb and Van Horn, for example, have constructed a detailed, semi-quantitative model of such a star which has a mass of 1.2 to 1.5  $M_{\odot}$  and can explain all the observed properties of Her X-1. It is known that degenerate dwarfs exist, and there seems every reason to believe that they will rotate, perhaps differentially. So where is the need to introduce such mathematical curiosities as black holes? Until all simpler explanations of these compact X-ray sources are eliminated, the relativists must look elsewhere for the conclusive proof that these singular predictions of their theories are correct.

Two astrophysicists who have done just that are Cunningham and Bardeen. They have calculated the detailed effect of a Kerr metric black hole on radiation emitted from a star of 1  $M_{\odot}$  which is in a circular orbit about the hole. A combination of Doppler shifts, gravitational redshifts and gravitational focusing combines to modulate the light from the star, as seen by a distant observer; the first two effects determine the surface brightness of the image seen, the third determines its size (*Astrophys. J.*, **183**, 237; 1973).

The problem is far from trivial. But its solution is worth the effort involved because it suggests a means for detecting black holes which is specific to them. No other system could produce a similar light curve, and as an added incentive if such a system were discovered by this signature the detailed variations of brightness and of the image size would provide a test of the strong-field predictions of general relativity.

A related investigation of this effect has been carried out by Maeder

(*Astron. Astrophys.*, **26**, 215; 1973). Both calculations show that the influence of black holes on companion stars should be detectable by present telescopes, and they should lead to an effort by observers to find such peculiarly varying systems.



Apparent position of the two brightest images (—; ---) as functions of time for one particular orbital radius and an observer at a polar angle of  $84.24^\circ$ . Numbered intervals define ten equally spaced times.

What should the observers look for? The actual light curve seen, of course, depends on the orientation of the orbital plane of the black hole/star system relative to the observer. In addition, the images produced will move around the sky as the star orbits the black hole and

its light is bent in different directions. And as a third mark of the presence of a black hole in such a system, more than one image is produced (see figure). It might not be possible to resolve the multiple images, which is a pity; but if there is a black hole at the centre of our Galaxy, as some people suggest, then there might well be a good chance of observing this kind of effect. Cunningham and Bardeen suggest in particular that the radio signal from a pulsar orbiting a black hole of some  $10^6 M_{\odot}$  at the galactic centre would show the required modulation. Extinction along the line of sight to the nucleus makes optical observation impossible, but the pulsar period would be so small compared with the orbital period of the neutron star about the black hole that the radio technique would be quite practicable.

Maeder prefers to concentrate on effects which might be produced when a black hole of a few solar masses and a star form a binary system, and he predicts that in some circumstances observers on Earth might see a brightening amounting to several tenths of a magnitude and lasting for several hours. In either case, these models and the observable effects they predict provide a much more realistic attack on the problem of the reality or otherwise of black holes than does the approach of invoking such singularities to account for observations which can also be explained by less extreme models.

## Acylated tRNA Metamorphoses

Sprinzel and Cramer, writing in *Nature New Biology* next Wednesday (September 5), suggest that tRNA is aminoacylated exclusively in the 2'-hydroxyl position, but that the aminoacyl group is transferred to the 3'-OH position before the reaction of peptidyl tRNA with aminoacyl tRNA in the ribosome.

Evidence for the isomerization of acylated tRNA during protein biosynthesis comes from the study of analogues of tRNA<sup>Phe</sup> lacking either the 2'- or 3'-OH group in the 3'-terminal adenosine. Only the tRNA<sup>Phe</sup>-C-C-3'dA can accept phenylalanine, exhibiting the same  $K_m$  and only a slightly lower  $V_{max}$  compared with normal tRNA<sup>Phe</sup>-C-C-A. tRNA<sup>Phe</sup>-C-C-2'dA is, however, a competitive inhibitor of the aminoacylation reaction. Such structurally-related molecules as periodate-oxidized tRNA<sup>Phe</sup>, tRNA<sup>Phe</sup>-C-C-A<sub>oxi</sub> and tRNA<sup>Phe</sup>-C-C are not competitive inhibitors.

Sprinzel and Cramer conclude that, although tRNA<sup>Phe</sup>-C-C-2'dA can be bound to the active site, aminoacylation can occur only when the hydroxyl group is in the 3' position.

In normal tRNA, acyl migration between 2' and 3' OH occurs with a half

time of 0.3 s. Thus peptidyl tRNA might donate its acyl group to the incoming aminoacyl tRNA by way of only one specific or either by way of both of these two positions on the ribose ring. Sprinzel and Cramer's evidence suggests that the ribose 2'-OH group is the sole effective acyl donor.

First, they cite earlier evidence that the 2' analogue of puromycin is inactive as an inhibitor of protein synthesis. This suggests that the 3'-peptidyl tRNA isomer is the only acyl donor *in vivo*. Their own experimental evidence is consistent with this view because they find that Phe-tRNA-C-C-3'dA cannot be utilized in a poly(U)-directed peptide synthesizing system *in vitro*. Free Phe-tRNA<sup>Phe</sup>-C-C-3'dA on the other hand can undergo hydrolysis (although three times slower than the normal tRNA). Transacylation must therefore be an obligatory step in protein synthesis.

This work suggests that there may be highly stringent steric requirements for the ribose isomers for both aminoacylation of tRNA by the twenty or so different synthetase enzymes and for formation of the peptide bond catalysed by peptidyl synthetase in the active site of the ribosome.

## EARTH'S MAGNETIC FIELD

**Seismically Excited?**

from our Geomagnetism Correspondent

THE best-known hypothesis for the origin of the geomagnetic field is that the field results from magnetohydrodynamic convective motions in the fluid outer core of the Earth. When this was proposed by Elsasser (*Phys. Rev.*, **69**, 106; 1946) nearly three decades ago, it became the first hypothesis consistent with the observed behaviour of the geomagnetic field at the Earth's surface and is still the most widely accepted view on the origin of the field today. The basic principle of core motion has rarely since been seriously questioned, although Malkus (*Science*, **160**, 259; 1968) alternatively proposed that the fluid circulation in the outer core may be due to the precessional torque on the Earth.

A basic assumption in hypotheses involving motions in the core is that the fluid is at its adiabatic gradient. Two years ago, however, Higgins and Kennedy (*J. geophys. Res.*, **76**, 1870; 1971) concluded that the adiabat for the outer core lies on the "wrong" (solid) side of the melting curve, so that if temperatures were actually distributed along the adiabat the outer core would be solid. They suggested instead that the temperature distribution in the outer core follows the melting point curve. One result of this is that the liquid in the core should be thermally stably stratified and that such stratification may inhibit fluid motion of the kind thought to generate the geomagnetic field. Subsequently, several workers claimed to have resolved this "core paradox" by suggesting that Higgins and Kennedy had miscalculated the adiabatic gradient, that they had wrongly estimated the melting curve of iron, that the outer core may not be pure iron anyway, or that the core comprises a slurry of fine iron particles in liquid suspension. But Kennedy and Higgins (*J. geophys. Res.*, **78**, 900; 1973) later refuted these suggestions, although they did concede that the lower third of the core may have temperatures distributed along an adiabat and thus be capable of sustaining free circulation.

Attempts are still being made to resolve the core paradox; and so for the time being doubt must remain as to whether or not the outer core can be set in motion by convective or precessional forces. In the meantime, however, and notwithstanding the claim by Kennedy and Higgins that their result will "impose severe limitations on any kind of circulation of the fluid in the outer core", Mullan (*Science*, **181**, 553; 1973) has proposed that earthquake waves may be sufficient to produce the motions necessary to the geomagnetic

dynamo. According to his calculations, if the total earthquake energy of  $10^{26}$ – $10^{27}$  erg yr<sup>-1</sup> were transferred to the whole liquid core as kinetic energy and if that kinetic energy were entirely dissipated in one year, the resulting motions would have an energy density of 1–10 erg cm<sup>-3</sup>. In fact, motions should last about 100 years against viscous damping if the velocity variations have length scales of the order of 500 km or more and assuming the kinematic viscosity to be  $\leq 10^6$  cm<sup>2</sup> s<sup>-1</sup>. Thus the kinetic energy available at any time may be derived from more than 100 years of earthquakes, giving energy densities of  $10^2$ – $10^3$  erg cm<sup>-3</sup>. Mullan suggests that energy densities in this range may be in equipartition with fields of 50–150 gauss.

Of course, all the energy from earthquakes will not reach the core in practice, although the fraction transmitted from mantle to core should presumably be greater than the  $10^{-4}$  that Won and Kuo (*J. geophys. Res.*, **78**, 805; 1973) calculated would reach the inner core from a large earthquake. But nor need the earthquake energy be transferred to the whole fluid core. Mullan thus concludes that  $10^{-4}$  of the earthquake energy transmitted to the outer 100 km of the outer core would still give an energy density high enough to achieve equipartition with a field of at least 5 gauss at the core-mantle boundary. As more general evidence that earthquakes have global effects, Mullan also points to recent proposals identifying earthquakes as the source of the Chandler wobble. Moreover, correlations of the length of the day with geomagnetic variations and independently with earthquake activity are claimed as indirect evidence for a correlation between geomagnetism and earthquakes.

Finally, Mullan draws attention to two possible implications of a seismic geomagnetic dynamo. The first relates to the obvious geographical correlation between the circum-Pacific seismic belt, where most of the deep earthquakes occur, and the Pacific Ocean, where the non-dipole field and secular variations are anomalously low. Could it be that the seismic excitation of core motions beneath the Pacific, being particularly intense, is sufficient to produce a "semi-permanent center of magnetic action" there with restricted drift?

Second, if the geomagnetic dynamo is excited seismically, the perturbations required to reverse the field may also be seismic. Mullan thus returns to the old idea that field reversals may be induced by the impacts of external bodies on the Earth. For example,  $10^{27}$  erg would be transmitted to the Earth by the impact of a meteorite of mass  $10^{15}$  g arriving at a velocity of 10 km s<sup>-1</sup>, and if the density of the meteorite were 10 g cm<sup>-3</sup> its radius would be  $3 \times 10^4$  cm. Mullan calculates

that the average time interval between the impacts of meteorites of such a dimension is  $1.7 \times 10^5$  yr. This is remarkably close to the characteristic time between field reversals ( $2 \times 10^5$  yr) as deduced by Cox (*J. geophys. Res.*, **75**, 7501; 1970).

## ATOMIC MASS

**Errors in the Chain**

from a Correspondent

A DISCREPANCY with accepted atomic mass values has been observed in a recent measurement of the <sup>58</sup>Ni–<sup>55</sup>Co mass difference. In their report to *Physical Review Letters* (**30**, 1255; 1973) Goss, Browne and Rollefson of the University of Notre Dame, Indiana, suggest that there is now reason to doubt all accepted values in this mass region.

Atomic masses are frequently quoted with an uncertainty of less than one part in ten million. For some, this is the result of direct measurements with a mass spectrometer; for most, it follows from an elaborate averaging procedure that takes account of a variety of nuclear reaction data. These data link together the masses of many small groups of isotopes in diverse combinations throughout the periodic table. One isotope may appear in many combinations, so the determination of its mass depends in a complex way upon the masses of its neighbouring nuclei. An error in one mass often implies more widespread problems.

By bombarding <sup>58</sup>Ni with 14 and 15 MeV protons and accurately measuring the energy of the outgoing  $\alpha$ -particles, Goss *et al.* could relate the mass of the produced <sup>55</sup>Co atoms to that of the target atoms. Their result differs by more than three standard deviations from the value calculated from the most recent compilation of atomic masses (Wapstra and Gove, *Nuclear Data Tables*, **9**, 265; 1971). Such a discrepancy might not be particularly alarming were it not for the fact that a difficulty already exists in this mass region. Wapstra and Gove have pointed out that two sets of mass spectrometer measurements on <sup>57</sup>Fe disagree significantly with one another, and they warn that there could be a "rather large systematic error" in this whole region. It might well be that the Goss experiment is confirming that fear.

On the other hand, there may be a less traumatic explanation. An examination of the sources used to determine the tabulated <sup>55</sup>Co mass value reveals that, while several measurements are included, one has a claimed accuracy that is an order of magnitude better than the others. In effect, it is this value alone that determines the accepted <sup>55</sup>Co mass. The method used in that experiment was



to measure the total energy released in the beta decay of  $^{55}\text{Co}$ , that is the high energy limit of its electron spectrum (see Fischbeck *et al.*, *Phys. Rev.*, **150**, 941; 1966). Achieving high accuracy in such measurements is notoriously difficult. It may well be that there is merely a conflict between two individual experiments rather than a fundamental flaw in the chain of atomic masses.

## SOLID STATE PHYSICS

### Dense Optical Memory

from a Correspondent

OPTICAL circuits and optical memories may, in the future, play a major part in data transmission, data processing and other electronic equipment (see, for example, *Nature*, **244**, 135; 1973). The various elements necessary for optoelectronic data handling machines are appearing one by one as the research laboratories concerned with computer development compete to make their products faster and smaller. An author from IBM (Wieder, *Appl. Phys. Lett.*, **22**, 487; 1973) now reports further improvements in dense information storage by optical means. Of all the feasible techniques of data storage, optical data storage already has the best potential for high-density use; Wieder's new concept, termed "superresolution", further improves the position of optical storage relative to other forms such as those using magnetic bubbles and semiconductor circuits.

An important method of optical data storage is the local alteration of the order in an amorphous thin film or, alternatively, the local "amorphization" of a crystalline film (see, for example, von Gutfeld and Chaudhari, *J. Appl. Phys.*, **43**, 4688; 1973). The best way of heating the film locally is with a laser beam, and a junction laser is a particularly convenient type to use. Compared with other light sources, a laser can "write" a very small spot on a recording medium. It would, however, be advantageous to work with even smaller spots, in order to make optical memories even more compact. A junction laser gives a dark spot on a photographic film which is more than  $4\text{ }\mu\text{m}$  in diameter, but it would be desirable to reduce that by half so that the grid of spots could have a spacing of, say,  $3\text{ }\mu\text{m}$ . The intensity of the laser spot is not uniform from edge to middle, but is, in fact, a gaussian profile, with a width at half maximum of  $2.5\text{ }\mu\text{m}$ . The use of a threshold process, such as melting of a thin film, is a way of reducing the size of "spot" recorded. The power level of the beam can be adjusted so that only a small area, of diameter considerably less than the half width of  $2.5\text{ }\mu\text{m}$ , is melted. In this way, spots less than  $2.0\text{ }\mu\text{m}$  across have been obtained, lead-

ing to a correspondingly dense packing of information.

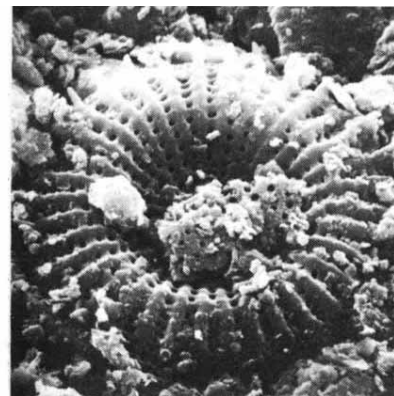
Now, however, comes the problem of reading out this densely packed information. The "read" beam is again gaussian, and will also generate diffraction patterns when interacting with the spot. Thus, if the "read" beam from the laser is again  $4\text{ }\mu\text{m}$  in diameter, it cannot separate the information in two adjacent spots that are of  $2\text{ }\mu\text{m}$ . Again, a threshold effect has to be found for the reading technique.

Wieder has come up with a very convenient one using the potentiality for threshold effects present in the reading laser itself. The laser is run at a level at which it is luminescing but slightly below the threshold current for lasing. When the beam sweeps over a more reflective area, such as the melted and recrystallized regions representing the "spots" of information, the increased light reflexion provides feedback into the laser resonant cavity and lasing commences. The edge of the spot can thus be detected either by sensing the altered light output of the laser or changes in the current drawn by the laser. The gaussian waveform of the read beam is thus effectively "clipped" by a large and adjustable factor.

In the system described by Wieder, a mixed chalcogenide film was used, containing tellurium, germanium and arsenic, with a preponderance of tellurium. It was evaporated to form a crystalline film, but a film of this composition is easily converted into the amorphous form by melting and rapid cooling. With the laser running continuously above threshold, that is without the use of the "clipping" technique, the system could not read dots written on  $3\text{ }\mu\text{m}$  centres. But when the laser was run just below threshold, so that the clipping action could operate, the same array of dots was clearly resolved. Extrapolating these trends, by plotting bit separation against modulation depth (essentially the resolving power of the system), it would seem that the threshold method should allow bit separations as low as  $2.5\text{ }\mu\text{m}$ , whereas the non-threshold scan should not achieve a separation of less than  $5\text{ }\mu\text{m}$ .

The development of the optical memory for computers has clearly come a long way from the form that is so familiar, namely the "IBM card". Bit separation was in the millimetre range for these, whereas it is in the micrometre range for the system now described by Wieder.

### Diatomites in Saharan Dust



THE Harmattan is a dust-laden wind which moves south from the Sahara across West Africa during the dry period from December to February. The small dust particles, which mostly range in size from  $0.1$  to  $1\text{ }\mu\text{m}$ , are deposited as the wind meets humid air from Guinea Bay.

The fact that these particles can very effectively penetrate and damage delicate electronic equipment led to a sample being collected from an air filter of a piece of standard ARN-2 equipment at the Ahmadu Bello University at Zaria, Northern Nigeria, and being sent for examination to Dr J. Buczkowski of Warsaw Technical University. Dr Buczkowski found that the dust included

very distinct shapes, some of which are typical of warm sea diatomites with dimensions of about  $10\text{ }\mu\text{m}$ . Debris particles were less than  $1\text{ }\mu\text{m}$ . The figures here show two of the photographs which he obtained with a JSM-U3 scanning electron microscope with a  $25\text{ kV}$  electron beam. The magnification was between 3,000 and 10,000.

Other workers have recorded diatomites in the Lake Chad area and Dr Buczkowski considers that study of dust particles originating from the Sahara might provide valuable information on the geological history of the region, as well as help to identify the origin of the Harmattan winds themselves.

# Nitrogen Oxides, Nuclear Weapon Testing, Concorde and Stratospheric Ozone

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E. L. SIMMONS & R. L. NEWSON

Meteorological Office, Bracknell

Although amounts of nitrogen oxides equivalent to the output from many Concorde were released into the atmosphere when nuclear testing was at its peak, the amount of ozone in the atmosphere was not affected.

At present the ozone layer, which is centred at a height of about 25 km in the stratosphere, is not completely understood. At one time it was thought that the oxygen reactions alone, the so-called Chapman classical theory, could account quantitatively for the observed ozone profiles. But as knowledge of the relevant rate coefficients and the natural distribution of ozone became more exact, it became apparent that the classical theory gave theoretical ozone profiles with a total ozone content much greater than that observed. This discrepancy was for a while accounted for by turning to the so-called "wet chemistry" thesis—that reactions involving water and its dissociation products were responsible for a higher ozone destruction rate than that of the classical theory. Again better measurements of the relevant chemical rate coefficients led to the fall of this explanation from favour. The current suggestion<sup>1,2</sup> is that naturally occurring nitrogen oxides in the stratosphere, possibly originating from the troposphere and high atmosphere, may play a significant role in the conversion of stratospheric ozone and atomic oxygen to molecular oxygen. It would seem<sup>3</sup> that a concentration of 1 to 10 parts per  $10^9$  of nitric oxide in the stratospheric air is required to produce agreement between measurement and the nitrogen oxide photochemical theory. The actual amounts of nitric oxide in the stratosphere have yet to be established.

It is in this context that there has been much speculation recently<sup>3-5</sup> that the oxides of nitrogen produced in the combustors of the jet engines of high flying supersonic aircraft could interact with, and so attenuate, the Earth's ozone shield and increase the ultraviolet radiation reaching the planetary surface. It is fair to say that none of these papers contains the necessary full, quantitative consideration<sup>6</sup> of the interaction of radiation, photochemistry and the atmospheric circulation. It is, however, incontrovertible that in laboratory conditions NO and NO<sub>2</sub> convert ozone and oxygen atoms to molecular oxygen. Such considerations have resulted in the initiation of a number of research programmes in the United States, Britain and France to study the climatic consequences of the operation of fleets of supersonic aircraft.

Any process which heats air above  $\sim 2,300$  K will produce significant quantities of NO as an equilibrium constituent. Examples of such heating include the shock waves from nuclear explosions as well as the operation of the combustion chambers of aircraft jet engines. The actual amount of nitrogen oxides remaining at ambient temperatures will

depend on the rate at which the heated air cools through the 2,500 K temperature region. It may be that massive amounts of nitrogen oxides have already been injected into the stratosphere during the various nuclear testing programmes.

## Shock Wave

In order to calculate the amounts of nitrogen oxides produced in the shock waves of nuclear explosions, it is necessary to know the pressure-temperature history of each parcel of air through which the shock front passed. We used the mathematical description of the spherical shock wave given by Taylor<sup>7</sup> to calculate the pressure-temperature histories of seven spherical surfaces enclosing fixed masses of air around the explosion centre. We then obtained values of pressure and temperature of each spherical shell enclosed by these seven surfaces by averaging the values at the two boundaries. The innermost shell (1), which includes the explosion centre, was taken to have the temperature and pressure history of the first spherical boundary. This was carried out for explosions of 1, 10 and 60 Mton TNT equivalent. The results of these calculations are given in Fig. 1.

Zel'dovich and Raizer<sup>8</sup> point out that the theory is not strictly applicable to the central regions of the explosion (shells (1) and (2)) where radiative effects probably dominate. Also the theory becomes more approximate when the pressure in the shock front is less than about ten times that of ambient.

In arriving at the curves in Fig. 1, a constant value, 1.4, of the ratio of specific heats  $\gamma$  has been assumed. Although at higher temperatures,  $\gamma$  is reduced from its ordinary tropospheric value of 1.4 by the absorption of energy in the form of molecular vibrational quanta, at very high temperatures increasing dissociation may be expected to compensate for this effect. Taylor noted that his calculations fitted the observed behaviour of the fireball in the first nuclear test in New Mexico better for  $\gamma=1.4$  than for  $\gamma=1.3$ . The shock wave has been assumed to propagate spherically in a uniform atmosphere at 290 K and 1,000 mbar. For the largest explosion considered (60 Mton), the radius of the 2,000 K surface is about 3 km, at which height this approximation is acceptable as demonstrated by Korobeinikov's more accurate description<sup>9</sup> of shock wave propagation in a real atmosphere.

## Chemical Kinetics of Heated Air

The production of NO in shock-heated air has been directly measured in laboratory shock tube experiments at temperatures between 2,300 K and 6,000 K by Camac and Feinberg<sup>10</sup>.



Mole fractions of nitric oxide,  $[\text{NO}]/[\text{M}]$ , of up to 0.1 were observed; the experiments applied, however, to short reaction times (several microseconds) and pressures from 0.01 to 0.07

a particular air mass, through its pressure-temperature history as a function of time down to times and temperatures where the NO production and destruction rates are slow compared to the cooling rate.

The elementary chemical processes used in calculating the nitrogen oxide production in the hot air masses created by the nuclear shock wave are as follows.

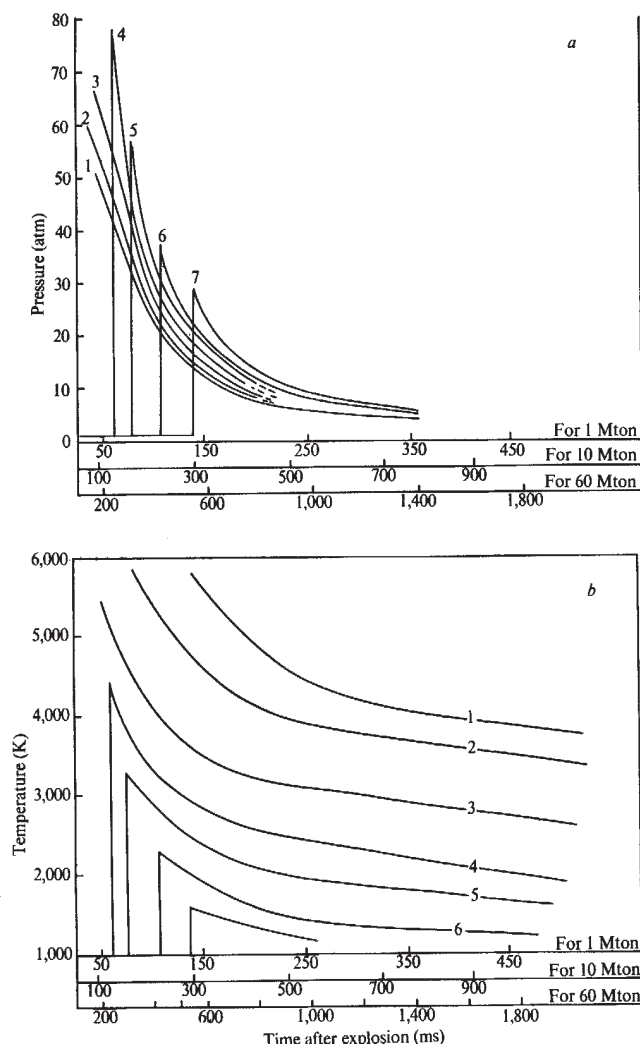
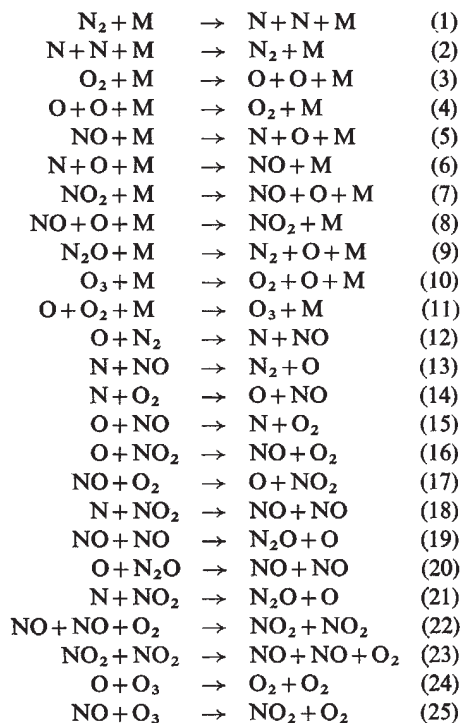


Fig. 1 Pressure (a) and temperature (b) histories for seven shells due to the shock wave around 1, 10 and 60 Mton explosions. Masses in units of  $10^{12}$  g per shell are

| Shell No | 1 Mton | 10 Mton | 60 Mton |
|----------|--------|---------|---------|
| 1        | 0.115  | 1.15    | 6.9     |
| 2        | 0.115  | 1.15    | 6.9     |
| 3        | 0.103  | 1.03    | 6.2     |
| 4        | 0.075  | 0.75    | 4.5     |
| 5        | 0.145  | 1.45    | 8.7     |
| 6        | 0.273  | 2.73    | 16.4    |
| 7        | 0.283  | 2.83    | 17.0    |

atm. This is in contrast to the conditions following a weapon burst, where, after a few hundred milliseconds, pressure relaxation rates are of the order of  $50 \text{ atm s}^{-1}$  and cooling rates are of the order of  $2,000 \text{ K s}^{-1}$ . The masses of the concentric shells of air heated above 2,000 K are typically of the order of  $10^{12}$  g. In these circumstances the best *a priori* way to calculate production of nitrogen oxides would be to start with a chemical kinetic mechanism composed of the elementary processes thought to be relevant and derive the set of simultaneous differential equations describing the variation in time of the number densities of the molecular species involved. These equations should then be solved, by numerical integration, for



Excited species of both atoms and molecules have not been included, though some, for example  $\text{O}(^1\text{D})$ ,  $\text{O}(^1\text{S})$ ,  $\text{O}_2(^1\Delta)$ ,  $\text{O}_2(^1\Sigma_g^+)$ ,  $\text{N}^2(\text{D})$ ,  $\text{N}_2(\text{A}^3\Sigma_u^+)$ ,  $\text{N}_2(\text{B}^3\Pi_u)$ ,  $\text{NO}(\text{A}^4\Pi)$  and  $\text{NO}(\text{A}^2\Sigma^+)$ , may be expected to be populated or chemically produced at the higher temperatures. Justification for this omission is that the pressures which accompany the high temperature ensure that collisional deactivation of excited species is likely to be efficient. Also possible coupling between molecular motions, chemical processes and the kinetic energy of the shock wave in the air immediately behind the shock front has been considered to be negligible because a comparison of the time taken for the shock to traverse the thickness of the shock front (a few mean free paths) and the time scale of the chemistry indicates that absorption of energy from the shock by chemical processes has but a small effect upon the shock front velocity in those regions where radiation effects are unimportant.

The integrations were carried out using rate coefficients taken from recent authoritative reviews as summarized in Table 1. With the exception of reactions (3) and (4), the rate coefficients have been adjusted for molecular nitrogen as the third body M. For reactions (3) and (4), the larger third-body efficiencies of atomic and molecular oxygen were used. Where the reverse process of a reaction occurred, values were chosen to be compatible with the thermochemical equilibrium constant.

The integrations were carried out by a semi-implicit scheme, programmed in ASSEMBLER, a low level IBM language. Initially, a simple Euler first-order forward time step method was used to perform the integration. A prohibitively small time step of  $10^{-10}$  s was required by this method. By setting nitrogen atoms and ozone to equilibrium, this was lowered to  $5 \times 10^{-7}$  s for shell (6), with its temperature of 2,300 K. The semi-implicit method could run satisfactorily, without setting any species to equilibrium, using a timestep of  $10^{-6}$  s at 2,300 K,

$5 \times 10^{-7}$  s between 2,300 K and 3,300 K (shell (5)), and  $10^{-7}$  s between 3,300 K and 4,400 K (shell (4)). The results obtained by the different methods at 2,300 K agreed closely; to have run the forward time step method at higher temperatures would have involved setting nitric oxide itself to equilibrium, thus defeating the purpose of the exercise. The semi-implicit scheme used the relation

$$Y_{n+1}(i) = \frac{Y_n(i) + P(i)\Delta t}{1 + Q(i)\Delta t}$$

where  $Y_{n+1}(i)$  is the concentration of the  $i$ th constituent at the  $(n+1)$ th time step and  $P(i)$  and  $Q(i)$ , functions of rate coefficients and concentrations of other species, are defined by the general chemical kinetic differential equation

$$dY(i)/dt = P(i) - Q(i) Y(i)^m$$

where in this case  $m=1$  or 2. At every time step each species was advanced in turn by this method, while the remainder were held constant. This method does not simultaneously solve all the continuity equations, and so cannot be guaranteed

**Table 1** Rate Coefficients

| Reaction | $A$                   | $N$  | $E$     | Reference |
|----------|-----------------------|------|---------|-----------|
| 1        | $3.73 \times 10^{21}$ | -1.6 | 224,900 | 11        |
| 2        | $2.25 \times 10^{21}$ | -1.6 | 0       | 11        |
| 3        | $4.79 \times 10^{18}$ | -1.0 | 118,700 | 13        |
| 4        | $2.38 \times 10^{17}$ | -1.0 | 340     | 13        |
| 5        | $2.27 \times 10^{17}$ | -0.5 | 148,830 | 14        |
| 6        | $6.45 \times 10^{16}$ | -0.5 | 0       | 14, 12    |
| 7        | $1.54 \times 10^{16}$ | 0    | 65,000  | 14        |
| 8        | $1.47 \times 10^{15}$ | 0    | -1,870  | 14        |
| 9        | $1.30 \times 10^{15}$ | 0    | 58,000  | 15        |
| 10       | $3.89 \times 10^{14}$ | 0    | 22,720  | 13        |
| 11       | $6.46 \times 10^{12}$ | 0    | -2,100  | 13        |
| 12       | $1.36 \times 10^{14}$ | 0    | 75,400  | 14        |
| 13       | $3.10 \times 10^{13}$ | 0    | 334     | 14        |
| 14       | $6.43 \times 10^9$    | +1.0 | 6,250   | 14        |
| 15       | $1.55 \times 10^9$    | +1.0 | 38,640  | 14        |
| 16       | $1.00 \times 10^{13}$ | 0    | 600     | 14        |
| 17       | $1.00 \times 10^{12}$ | 0    | 45,500  | 14        |
| 18       | $3.60 \times 10^{12}$ | 0    | 0       | 14, 16    |
| 19       | $9.40 \times 10^{11}$ | 0    | 66,000  | 14        |
| 20       | $2.50 \times 10^{13}$ | 0    | 26,900  | 14        |
| 21       | $1.20 \times 10^{12}$ | 0    | 0       | 14, 16    |
| 22       | $2.43 \times 10^9$    | 0    | -1,046  | 14        |
| 23       | $4.00 \times 10^{12}$ | 0    | 26,900  | 14        |
| 24       | $1.20 \times 10^{13}$ | 0    | 4,790   | 13        |
| 25       | $5.72 \times 10^{11}$ | 0    | 2,460   | 34        |

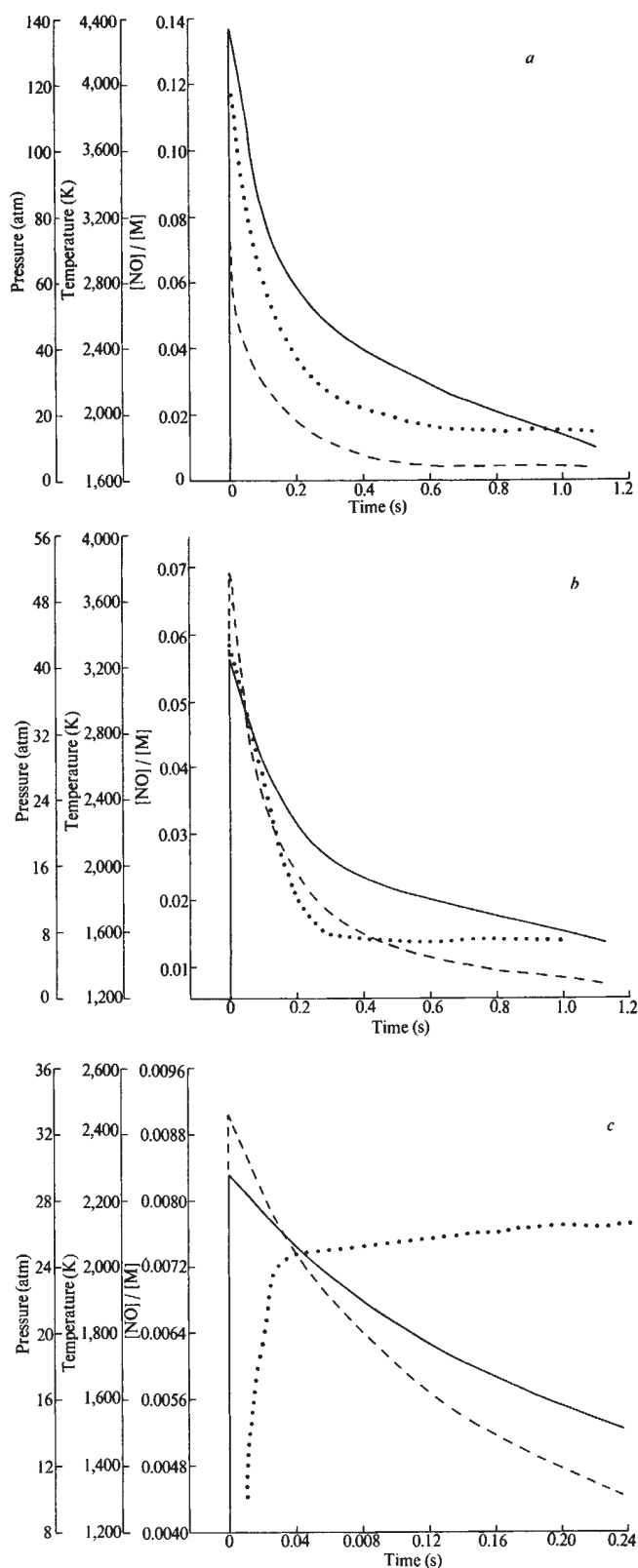
$$k = AT^n \exp(-E/1.987T).$$

$k$  is in  $(\text{cm}^3 \text{mol}^{-1})^n \text{s}^{-1}$  units;  $E$  is in calorie  $\text{mol}^{-1}$ . ( $n=1$  for a two-body process and  $n=2$  for a three-body process).

to conserve chemical identity (that is to say the total numbers of oxygen and nitrogen atoms). Chemical conservation was in fact obeyed quite well except in the few integrations which went to numbers of time steps greater than about  $2 \times 10^6$ . In all cases, however, results are quoted from integrations where conservation of N atoms and of O atoms was ensured by scaling after each time step to the density implied by the pressure and temperature history derived from Taylor's equations. Results of typical integrations are shown in Fig. 2. The rate coefficients were recalculated at descending 50 K intervals, after assuming a step perturbation of air from STP to the initial pressure and temperature values on each of the curves (4), (5) and (6) of Fig. 1. The air in the outer shell (7) (curve (7)) never reaches a high enough temperature to produce significant amounts of NO.

The results of these calculations are given in Table 2 in which the nitric oxide yield for each concentric shell is listed, together with the total air mass in that shell for explosions of 1, 10 and 60 Mton equivalent TNT.

It is seen that the NO yields for 1, 10 and 60 Mton explosions are  $0.49$ ,  $5.4$  and  $35.5 \times 10^{10}$  g respectively. The yield in the range 1 to 10 Mton is about linear, with  $5 \times 10^9$  g of NO being



**Fig. 2** Nitric oxide concentrations as a function of time for a 10 Mton explosion. *a*, Shell (4), shock starts 155 ms after ignition; *b*, shell (5), shock starts 197 ms after ignition; *c*, shell (6), shock starts 273 ms after ignition. The pressure and temperature histories are also plotted. —, Temperature; ---, pressure; ..., [NO]/[M].



produced per Mton of explosive energy. This linearity breaks down somewhat for the 60 Mton explosion because in the outermost of the shells producing significant amounts of NO (shell (6)), the NO production is slow enough to be dependent on the time spent at high temperatures, rather than the cooling rate as is the case for the inner shells. Sixty megatons is about the size of the largest individual nuclear explosion ever made, in Russia in 1962.

The deduced production of  $5 \times 10^9$  g per Mton of explosive power can be compared with the findings of three previous studies.

The first by Davies<sup>17</sup> was primarily concerned with the chemical composition of the air in the shock wave and how the resulting changes in  $\gamma$  would affect the shock propagation. He did, however, conclude on the basis of a statistical mechanical treatment of equilibrium constants that at 4,000 K 4.6% of nitric oxide would be present. He also concluded that no NO would be produced at temperatures of 3,000 K or lower; that is because the statistical mechanical approach does not allow for the changes in the chemical reaction rates with temperature being slower than the rate of change of temperature. The amount of NO left after the passage of the shock wave was not of concern in this study.

**Table 2** Calculated NO Production

| Explosion strength (Mton) | Shell No. | Air mass in shell ( $10^{12}$ g) | [NO]/[M] | [NO] ( $10^{10}$ g) | [NO] ( $10^{32}$ molecules) |
|---------------------------|-----------|----------------------------------|----------|---------------------|-----------------------------|
| 1                         | 4         | 0.075                            | 0.017    | 0.13                | 0.26                        |
|                           | 5         | 0.145                            | 0.016    | 0.23                | 0.46                        |
|                           | 6         | 0.270                            | 0.005    | 0.13                | 0.26                        |
|                           | Total     |                                  |          | 0.49                | 0.98                        |
| 10                        | 4         | 0.75                             | 0.015    | 1.2                 | 2.4                         |
|                           | 5         | 1.45                             | 0.014    | 2.1                 | 4.2                         |
|                           | 6         | 2.70                             | 0.008    | 2.1                 | 4.2                         |
|                           | Total     |                                  |          | 5.4                 | 10.8                        |
| 60                        | 4         | 4.5                              | 0.015    | 6.8                 | 13.6                        |
|                           | 5         | 8.7                              | 0.013    | 11.1                | 22.2                        |
|                           | 6         | 16.4                             | 0.011    | 17.6                | 35.2                        |
|                           | Total     |                                  |          | 35.5                | 71.0                        |

In the second, Zel'dovich and Raizer<sup>18</sup> assume that all the air that had been taken to temperatures greater than 2,300 K in the shock wave would ultimately have equilibrium NO concentrations frozen at values characteristic of 2,300 K when returned to ambient temperatures. On this basis they concluded that a 20 kton equivalent TNT explosion would yield a concentration of NO of about 1%, equivalent to about 50 ton. On a linear extrapolation this means that a 1 Mton explosion would yield  $2.5 \times 10^9$  g. This is about half the yield indicated by the more comprehensive calculations outlined here. In the third study, Foley and Ruderman<sup>19</sup>, simply took one-third of the liberated explosive energy yield as remaining as thermal energy in the buoyant hot air and assumed that this energy would be used in heating a mass of air to 2,000 K. The physical basis of this assumption is not readily apparent from detailed studies of Taylor<sup>7</sup> and Brode<sup>20</sup>. They further assumed that all this air instantaneously produced a concentration of 0.8% NO. The time scale for NO production for curve (6) of Fig. 2 of this article demonstrates one limitation of this approach. However, this simplified treatment of the physics and chemistry of nuclear explosions yielded a similar result ( $10^{10}$  g of NO per Mton explosive energy), higher than that of our method by a factor of two.

What are the limitations of the treatment presented here? In arriving at the nitric oxide production rate, the inner three shells, denoted in Fig. 1, and containing 0.33, 3.3 and  $20 \times 10^{12}$  g, respectively, for 1, 10 and 60 Mton events have not been included. This is not only because of the inapplic-

ability of the Taylor formulations to these inner regions where radiative transfer may be relatively important but it might also be argued that these high temperatures gases absorb the ionizing radiations from the innermost region, so shielding the nitric oxide in the outer shells (4), (5) and (6). The chemistry of a hot ionized air mass, subjected to radioactive emissions, is not easily modelled. Any nitric oxide production in these innermost shells will be additional to  $5 \times 10^9$  g per Mton deduced from the outer shells. Also, all the calculations apply to a spherical shock wave. For explosions on or near the ground some of the energy would be absorbed in the Earth rather than propagated in the atmosphere, leading to a somewhat smaller nitric oxide production.

As mentioned earlier, a value of 1.4 for  $\gamma$ , the ratio of specific heats, was used in calculating the pressure-temperature histories of the shock wave. To test the effect of decreasing  $\gamma$ , the pressure-temperature histories were recalculated for a 10 Mton event using  $\gamma=1.3$ . The nitric oxide production was now attributed to shells (3), (4) and (5) rather than (4), (5) and (6). It was found that the total nitric oxide production was decreased to  $4.5 \times 10^9$  g per Mton.

The overall calculated NO production for an explosion was found not to be very sensitive to variation to any one of the rate coefficients  $k_3, k_4, k_{12}, k_{13}, k_{14}, k_{19}$  and  $k_{20}$  within their experimental limits. But simultaneously setting the first six of these ( $k_3$  to  $k_{13}$ ) to their lower limits and the other two ( $k_{19}$  and  $k_{20}$ ) to their upper limits reduced total nitric oxide production to  $2.7 \times 10^9$  g per Mton. Inspection of the course of the integrations showed that reactions (3), (12) and (14) were the chief producers of nitric oxide and reaction (19) was the principal destroyer. This is in agreement with the experimental observations of Camac and Feinberg. Thus the value of  $2.7 \times 10^9$  g per Mton is a lower limit in the light of current knowledge of rate coefficients.

The amounts of NO<sub>2</sub> and O<sub>3</sub> generated by explosions also formed part of the solutions of the differential equations. It was found that NO<sub>2</sub> was always less than 1% of the nitric oxide production and that the O<sub>3</sub> production was negligible. The fact that the NO<sub>2</sub> amounts were so small provides some justification for omitting the higher oxides of nitrogen (NO<sub>3</sub>, N<sub>2</sub>O<sub>5</sub>). In these calculations, the temperature history below 1,500 K was not considered. Needless to say there will be considerable conversion of NO to NO<sub>2</sub> as the air mass cools to ambient. This would account for the observations<sup>22</sup> of an orange colour to the nuclear cloud at high altitudes.

The chemical model outlined here is strictly speaking applicable only to dry air. In the real atmosphere, it is possible that a number of reactions involving water and its dissociation products should be incorporated. It is difficult to visualize a chemical mechanism at the relevant elevated temperatures by which water or its dissociation products at atmospheric mixing ratios could prevent the formation of NO. Species such as HNO, HNO<sub>2</sub>, HNO<sub>3</sub> have dissociation energies less than NO<sub>2</sub>, thus in the absence of some catalytic destruction of NO or NO<sub>2</sub> by H, OH, HO<sub>2</sub> and H<sub>2</sub>O processes such as



cannot prevent the formation of NO in the shocked air masses. Further calculations on this point are in progress, but it is a fact that in the emissions of jet engines the gases contain high amounts of water and dissociation products and this does not apparently inhibit production of NO.

## Nitrogen Oxide Production by Concorde

The recommended value<sup>21</sup> for the emission of nitrogen oxides from the Concorde's Olympus Mark 602 engines is 13.8 g of NO per kg of fuel consumed, which is rather larger than

estimates based on measurements made at the National Gas Turbine Establishment, Pyestock. At a cruise altitude of about 55,000 foot, the engine's fuel consumption is about 4,550 kg h<sup>-1</sup> per engine. Thus assuming a trans-Atlantic crossing requires 2½ h flying time in the stratosphere, the total nitrogen oxide production per flight expressed in terms of NO is 670 kg. Thus one Concorde crossing the Atlantic, four times a day each day of the year produces  $9.8 \times 10^5$  kg yr<sup>-1</sup> of NO.

Table 3 gives a year by year summation of all nuclear explosions<sup>19,22-24</sup> greater than 20 kton TNT together with the associated NO production deduced from the calculated yield of  $5 \times 10^9$  g per Mton. In the fourth column is the number of Concorde that would have to fly across the Atlantic four times a day each day of the year to produce the same amount of NO as that year's nuclear weapon testing.

It is seen that the nuclear testing is equivalent during the period 1952 to 1958 to more than 100 fully operational Concorde. During 1961 and 1962, as many as 800 Concorde would have had to have been fully operational throughout the two years for the same gross NO injection. The assumption is made that all nitrogen oxides produced in explosions are ultimately transported into the stratosphere. As the greater proportion of radioactive debris in an air burst of 1 Mton or greater is injected into the stratosphere<sup>25</sup>, it is not unreasonable to assume that all nitrogen oxides will reach the stratosphere in a similar manner. Although Table 3 gives a summation of nuclear explosions greater than 20 kton TNT, the total yield of explosions between 20 kton, which probably do not inject material into the stratosphere<sup>25</sup>, and 1 Mton, which probably do, is only a small proportion of the total. The inclusion of these low yield explosions will make no significant differences to the estimates of the stratospheric injection of nitrogen oxides.

**Table 3** Calculated Annual NO Production from all Nuclear Tests greater than 20 kton and the Equivalent Number of Fully Operational Concorde

| Year | Mton TNT | NO production 10 <sup>7</sup> kg | Equivalent No. of fully operational Concorde |
|------|----------|----------------------------------|----------------------------------------------|
| 1952 | 11.1     | 5.5                              | 54                                           |
| 1953 | 0        | 0                                | 0                                            |
| 1954 | 48.5     | 24.2                             | 237                                          |
| 1955 | 2.0      | 1.0                              | 10                                           |
| 1956 | 26.0     | 13.0                             | 127                                          |
| 1957 | 13.5     | 6.7                              | 66                                           |
| 1958 | 61.9     | 30.9                             | 303                                          |
| 1959 | 0        | 0                                | 0                                            |
| 1960 | 0        | 0                                | 0                                            |
| 1961 | 120.6    | 60.3                             | 591                                          |
| 1962 | 213.5    | 106.7                            | 1,047                                        |
| 1963 | 0        | 0                                | 0                                            |
| 1964 | 0        | 0                                | 0                                            |
| 1965 | 0        | 0                                | 0                                            |
| 1966 | 1.4      | 0.7                              | 7                                            |
| 1967 | 3.5      | 1.7                              | 17                                           |
| 1968 | 7.6      | 3.8                              | 37                                           |
| 1969 | 3.0      | 1.5                              | 15                                           |
| 1970 | 6.1      | 3.0                              | 30                                           |
| 1971 | 1.6      | 0.8                              | 8                                            |
| 1972 | 0        | 0                                | 0                                            |

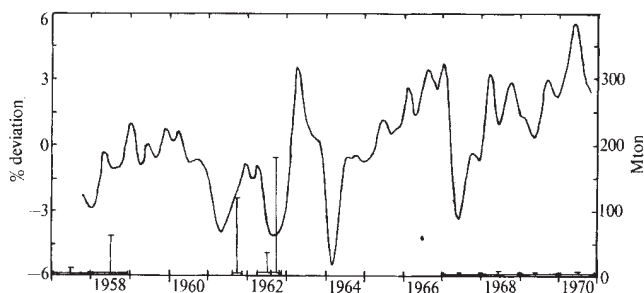
There will, however, be obvious differences between the stratospheric distribution due to aircraft emissions and that due to more rapid injections by explosions. In the first place, the aircraft injections are confined to the limited region of air routes, and operational flight altitudes in the lower stratosphere. The nuclear injections cover a much wider altitude range, depending on the strength of the explosion; the cloud of debris from a 1 Mton event typically stabilizes<sup>25</sup> at a height of about 22 km whereas that of a 10 Mton event would

reach about 32 km. Thus the Concorde emissions at the operational height of 17 km are always below the maximum in the vertical ozone profile, while the nitrogen oxides from nuclear explosions could well stabilize in the same height range as the ozone maximum.

It must therefore be accepted that amounts of NO at least comparable with those expected from large fleets of Concorde aircraft have already been directly injected into the stratosphere. If the atmospheric ozone budget is sensitive to injections of anthropogenic nitrogen oxides then one would expect the effects of the nuclear testing programme to be reflected in the past records of total ozone amounts. This applies especially to 1961 and 1962.

## Ozone Records

The total amount of ozone in the atmosphere above any point on the Earth's surface is highly variable, with large fluctuations from day to day and with a well established annual cycle which has a maximum in the early spring and a minimum in the late autumn. If the nitrogen oxides from the testing of nuclear weapons have affected total amounts of ozone, then one would expect this to be reflected in the ozone records since 1952, with special emphasis on the years 1961 and 1962 when weapons yielding 334 Mton were exploded, more than half the total explosive energy of all tests. Those tests before 1952



**Fig. 3** Smoothed deviations from monthly means of total ozone from seventeen stations in the period 1957 to 1970 and the intensity and timetable of nuclear explosions in this period. —, Deviation; ⊥, nuclear events.

were all of less than 1 Mton yield and contributed little to the total stratospheric injections.

In Fig. 3 are plotted ozone data from the seventeen stations in the northern hemisphere with the longest continuous record (see Table 4). The deviation from the monthly mean, expressed as percentage of that mean, is plotted monthly as a function of time. In this way the annual and short term fluctuations are removed. Also the mean of the seventeen stations was subjected to conventional five term binomial smoothing.

The stations were selected on the following criteria. The records exist from 1957 or 1958, the beginning of the IGY period, when many more stations commenced operation; they do not have gaps of more than 18 consecutive months in their data; and they have more than 110 monthly mean values for the total period 1957 to 1970. Stations making measurements with the Russian filter ozonometer were excluded because it has been demonstrated<sup>26</sup> that results from these instruments differ significantly from those of the Dobson ozone spectrophotometer for solar zenith angles greater than 60° or in the presence of haze. This may be important on account of



secular changes in haze (R. N. Kulkarni, private communication). Furthermore only ozone measurements in the northern hemisphere are considered because most nuclear weapon debris has been injected there. In any case very few stations in the southern hemisphere would have met the criteria of long term continuity. The periods of weapon testing together with total megaton yield for each of nine periods are also indicated on Fig. 3. The American equatorial and Russian high latitude tests of 1961–62, are separated on Fig. 3, with the American tests coming between the two periods of Russian tests.

It might be expected that if nitrogen oxides from the nuclear explosions are to modify the total ozone amount, then this would be reflected in the records, covering the northern

**Table 4** Ozone Records of the Stations Used in Fig. 3

| Station                      | No. of months | Mean total ozone | Slope (% per decade) | Error of slope ( $\sigma$ ) |
|------------------------------|---------------|------------------|----------------------|-----------------------------|
| Arosa (47° N 10° E)          | 158           | 334              | 1.2                  | 0.9                         |
| Cagliari Elmas (39° N 9° E)  | 161           | 329              | 8.6                  | 0.9                         |
| Camborne (50° N 5° W)        | 115           | 338              | 6.3                  | 1.7                         |
| Edmonton (54° N 114° W)      | 162           | 357              | 2.9                  | 1.0                         |
| Kagoshima (32° N 131° E)     | 151           | 289              | 1.9                  | 1.2                         |
| Kodaikanal (10° W 77° E)     | 161           | 255              | 10.6                 | 0.5                         |
| Lerwick (60° N 1° W)         | 151           | 348              | 7.2                  | 1.2                         |
| Messina (38° N 16° E)        | 141           | 346              | 3.5                  | 0.9                         |
| Napoli (41° N 14° E)         | 137           | 301              | -1.3                 | 1.1                         |
| New Delhi (29° N 77° E)      | 159           | 270              | 7.8                  | 1.1                         |
| Oxford (52° N 1° W)          | 162           | 353              | 4.3                  | 1.0                         |
| Resolute (75° N 95° W)       | 143           | 390              | 3.6                  | 1.8                         |
| Reykjavik (64° N 22° W)      | 134           | 339              | -1.7                 | 1.9                         |
| Sapporo (43° N 141° E)       | 155           | 371              | 1.0                  | 0.8                         |
| Tateno (36° N 140° E)        | 160           | 325              | 0.0                  | 0.9                         |
| Tromsø (70° N 19° E)         | 121           | 331              | 0.6                  | 2.0                         |
| Vigna Di Valle (42° N 12° E) | 162           | 341              | 1.1                  | 0.9                         |
| Mean of stations             | 162           | 330              | 6.3                  | 1.7                         |

hemisphere, delayed by a period dependent on stratospheric mixing times and relevant chemical reaction rates. It seems to us that these records do not provide evidence for such a modification.

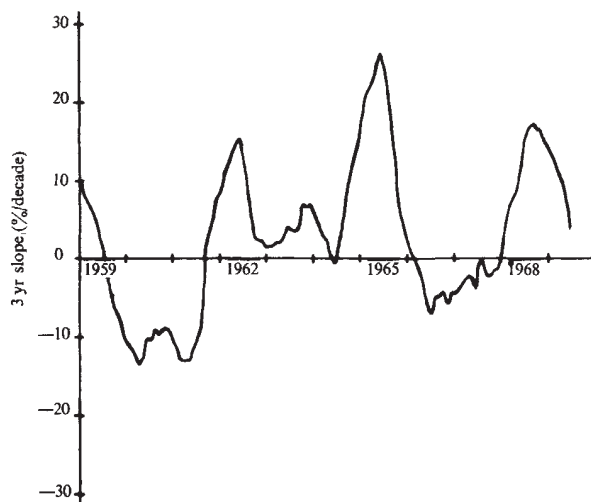
It has, however, been suggested by Johnston *et al.*<sup>27</sup> on the basis of an analysis of the records of ozone measuring stations from 1960, including those in the Soviet Union and some in the southern hemisphere, that in the northern hemisphere there was a significant decrease ( $-7.6\%$  per decade) in ozone during 1960–62, followed by an increase ( $+5.6\%$  per decade) in 1963–70. Johnston *et al.* argue on the basis of these figures, that “the oxides of nitrogen from nuclear bomb tests of 1952–62 constituted a measurable injection and the consequent reductions of ozone may be ascribable (perhaps only in part) to this injection experiment”. They also speculate that the ozone increase in 1963–70 may represent a recovery from the effects of the test period.

The ozone data going back to late 1957 given in Fig. 3, do not support the contention that the period 1960–62 is of any particular significance with respect to the periods of nuclear testing. Moreover there is considerable danger in comparing ozone trends over an interval of 3 yr. This is demonstrated in Fig. 4 in which the 3 yr slopes of the seventeen station ozone records are plotted monthly. It is seen that the slopes vary between about  $+26\%$  and  $-13\%$  per decade and so there is no significance to the  $-7.6\%$  per decade obtained by Johnston for 1960–62. Indeed during 1963–70, there was a period when the 3 yr slope was also negative.

The upwards tendency of the trace of Fig. 3 since 1961 agrees with the findings of Komhyr *et al.*<sup>28</sup> that there has been an increase of total ozone of up to  $10\%$  at a number of stations during the past decade (1961–70).

There have been a number of other studies of trends of ozone based on much longer records of data. Notably Willett<sup>29</sup> and Christie<sup>30</sup>, both on the basis of records going back to 1933,

detected a negative and largely unexplained correlation of total ozone with sunspot number. The ozone minimum precedes the sunspot maximum by about 20 months. London and Oltmans<sup>31</sup> were, however, unable to find such a correlation in the Arosa and Tromsø records, which formed the bulk of the Willett, Christie data before 1945. Paetzold *et al.*<sup>32</sup>, on the evidence of direct balloon-borne optical ozone soundings over central Europe between the years 1951 to 1972, report a positive correlation between sunspot number and the ozone amounts between 20 and 30 km. The evidence for this corre-



**Fig. 4** Percentage increase expressed in % per decade for the total ozone calculated from the deviations of the monthly means for seventeen stations over 3 yr intervals from 1959 to 1969.

lation is, however, not conclusive because of the lack of data in the first half of the period.

There are only two ozone observing stations with reasonably complete long term records. These are Arosa and to a lesser degree Oxford, both from 1926. In Fig. 5 the deviation from the monthly mean is shown for both these stations. The Oxford data between 1933 and 1944 have not previously been published, those for Arosa are given by Perl and Dütsch<sup>33</sup>.

These long term records demonstrate the high variability in total ozone over a wide variety of time scales and indicate the difficulty in detecting any short term trends. In common with other data presented here, there is nothing to suggest that nitrogen oxides from the testing of nuclear weapons in the atmosphere have had any effect on the total ozone.

## Implications

In this article the production of nitrogen oxides in the shock wave of explosions associated with the testing of nuclear weapons in the atmosphere has been calculated. This has allowed the total injection of nitrogen oxides into the stratosphere to be deduced for each year in which nuclear tests have taken place. These amounts have been compared with those expected from the engine exhaust products of fleets of Concorde each flying in the stratosphere for more than 10 h every day of the year. We conclude that past nuclear explosions have been equivalent, as far as nitrogen oxide stratospheric injections are concerned, to large numbers of these fully operational Con-

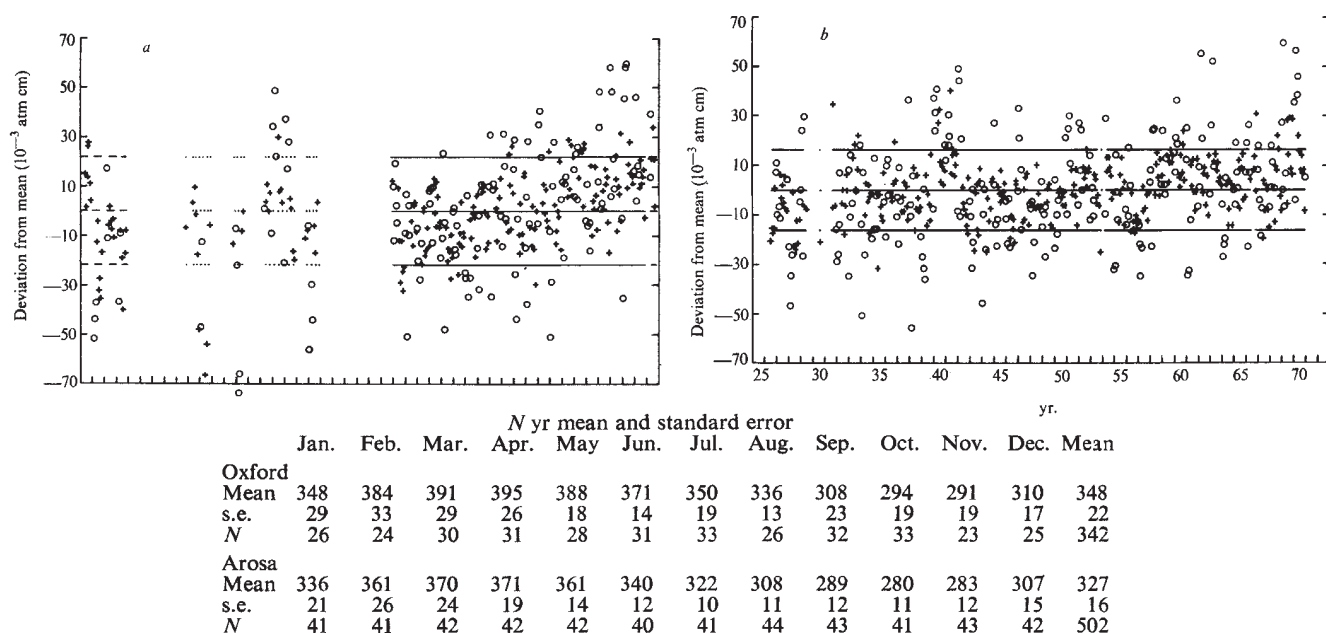


Fig. 5 Deviation of monthly total ozone from the mean of that month for Oxford, 52° N 1° W (a), and Arosa, 47° N 10° E (b), from 1925 to 1971. O, Values for January to March inclusive, when the total ozone is at its most variable; +, values for all other months. The units represent the equivalent thickness of ozone at standard temperature and pressure.

cordes. For instance, during the years of maximum nuclear activity, 1961 and 1962, the number of Concordes giving equivalent nitrogen oxide emissions would have been about 600 and 1,000 respectively. Analysis of the ozone records reveal no detectable changes in the total atmospheric ozone during and after the periods of nuclear weapon testing. Although the two modes of nitrogen oxide injection may not be identical from the meteorological view point, the conclusion that massive injections of nitrogen oxides into the stratosphere do not upset the ozone layer seems inescapable. Such a conclusion has profound implications for our understanding of

the photochemical processes in the stratosphere, as it is only by postulating an ozone sink due to natural nitrogen oxide at the level of 1 to 10 parts per 10<sup>9</sup> that current theories are able to account for the natural concentrations of ozone in the stratosphere. It may be that other factors such as solar radiation, stratospheric circulation, refinement of chemical rate coefficients and hitherto unconsidered chemical sinks may all play a part in bringing theory closer to measurement.

We thank Professor G. M. B. Dobson for making available the unpublished Oxford ozone records and R. Smart for help with analysis of the records.

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# Dynamic Mixing of Water and Lava

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The heat content of lava is such that if roughly equal volumes of lava and water are rapidly mixed, the resulting pressure may be enough to cause further dynamic mixing of a lava flow under the ocean leading to a catastrophic explosion. The same mechanism of self-sustained mixing may explain several foundry explosions of liquid metal and water, and others of large volumes of liquid natural gas and water where the water serves as the hot fluid. The Krakatau volcanic explosion, equivalent to 200 megatons, may have the same explanation.

DURING the Heimaey eruption it was suggested that an underwater lava flow threatening to close off the island's harbour might be dealt with by high explosive. The idea was that the resulting rapid mixing with water would thicken the lava and impede the flow. We planned such an explosion with the help of the Icelandic government, the Icelandic Coast Guard and the US Navy, but the day before the detonation we realized the awesome possibility that once mixing was initiated it might be self sustaining in that the high pressure steam produced might cause further mixing until all the lava had exchanged its heat with the water above it. The energy released might have come to between 2 and 4 megatons. Naturally the experiment was called off.

Since then we have found several references<sup>1-3</sup> to unexplained, but sometimes disastrous industrial explosions when molten aluminium, iron, or salt, came suddenly in contact with water. In no case was the release of chemical energy significant. In general, no detailed explanation is offered other than that the liquids must break up into small particles for rapid heat transfer<sup>4</sup>. In some cases, such as explosions of liquid natural gas (LNG) and water (when the water is the hot fluid and the LNG is the vaporizable liquid), it has been suggested<sup>5,6</sup> that delayed nucleation boiling may be involved.

We believe that all cases of contact between a hot fluid and a cold vaporizable fluid are potentially explosive by a process of dynamic self-sustained mixing. The amount of liquid necessary is associated with the Reynolds number of the mixing process, and the crust thickness, but not surface tension, is also important.

## Lava Crust Formation

When a lava flow enters the ocean, a crust protects the hot fluid from rapid cooling. There may be some additional cooling by water due to the formation of "pseudo-craters" and

cracks opening at the time and place of crustal extension, but once a competent crust is formed, the heat loss from the crustal surface is, by and large, governed by the thermal conductivity of the crust and not by the rate of heat exchange at the lava surface. The heat transport is described by a diffusion wave solution between a near constant temperature boundary and a semi-infinite region; the one water or atmosphere at  $T \approx 0$  to  $50^\circ\text{C}$  and the other hot lava at  $\approx 1,100^\circ\text{C}$ . The error function solution to the boundary value problem of thermal diffusion results in an effective "skin depth", the width of the thermal wave of

$$\delta = \sqrt{Dt} \text{ cm} \quad (1)$$

where  $D = K/C_v$  and where  $\delta$  is the crust thickness,  $C_v$  the heat capacity per unit volume,  $K$  the thermal conductivity and  $t$  the time.  $C_v$  is of order unity in c.g.s. units—corresponding to a heat capacity of approximately  $1/4$  calorie  $\text{g}^{-1} \text{ }^\circ\text{C}^{-1}$  and a density of  $2.8 \text{ g cm}^{-3}$  (ref. 7). The thermal conductivity is approximately  $5 \times 10^{-3}$  calories  $\text{cm}^{-2} \text{ s deg}^{-1} \text{ cm}^{-1}$  (ref. 8). The diffusivity,  $D = (K/C_v) \approx 10^{-2}$ , and the thermal skin depth in a week ( $t = 10^6 \text{ s}$ ) becomes  $\delta = 100 \text{ cm}$ . The similarity solution, equation (1), assumes only that the outer boundary is a constant,  $T_1$ , and that the inner boundary asymptotically approach  $T_2$ , and although an outer boundary temperature  $T_1$  in contact with seawater might be slightly cooler than an atmospheric boundary, this difference in outer boundary temperature is trivially small compared with the difference  $T_2 - T_1 \approx 1.1 \times 10^3 \text{ }^\circ\text{C}$ .

Therefore the crustal layer is thin compared with the thickness of the lava flow which may be 5 to 10 m thick on land and 20 to 60 m thick under the ocean<sup>9</sup>. Nevertheless, for relatively slow lava motions, which typically are less than  $100 \text{ m d}^{-1}$ , the crust effectively shields the lava from any violent mixing. Small eruptions, called pseudo-craters, may occur in a lava flow over ocean bottom, lake, or stream beds, but presumably these occur due to build up of pressure from the heating of a pocket of trapped water (Fig. 1).

A more catastrophic release of energy is conceivable if ocean water and a large undersea lava flow were to mix autocatalytically. Autocatalysis in this case implies that the pressure energy released by a given volume of water and lava is great enough to cause the further mixing and subsequent energy release of at least a slightly larger volume. In this fashion an exponential type of reaction takes place that increases until limited by depletion of the reservoir. The critical question is whether or not the gain is greater than unity because this determines whether an initiating disturbance grows or decays away.

## Underwater Explosions

When an explosive is detonated underwater, a shock wave propagates radially as a strong impulse of short duration that tends to fracture brittle objects, but does not significantly disturb semifluid media like sand or mud<sup>10</sup>. Behind the shock wave is a region of high velocity fluid flow that is uniformly



Fig. 1 Lava flow, volcano and town of Heimaey during the winter of 1973 is shown during the eruption of the volcano south of Iceland. The lava flow is entering the ocean and threatens to block the harbour entrance to the left. A pseudo-crater eruption is visible near the lava front where steam and finely divided lava are blown into the air as a grey cloud. We suggest that these small eruptions occur because of the entrapment of water beneath the flowing lava. Long<sup>1</sup> also suggests that the entrapment of water beneath molten aluminium poured into water is the mechanism for triggering his experimental explosions. (Photograph courtesy of Sigurgeir Jónasson and Icelandic Photo and Press Service.)

radial unless there is an interface present, such as that between ocean and ocean bottom. The flow velocity is larger in the less dense medium and by both the Helmholtz instability due to velocity shear, and Taylor instability from the acceleration of the heavy fluid by the light one, the light fluid penetrates the more dense medium and a mixture of mud, sand, rock, and water is ejected.

The quantity of sediment blasted free is almost always several hundred times the mass of explosive, and is roughly double the mass corresponding to a refilling of the crater void. The exact ratio depends on the details of the ocean sediments, but it holds within a factor of 2 from the smallest explosions to the largest nuclear bombs, provided only that the explosion is "stemmed" with the appropriate water depth in the ocean, or buried at the appropriate "scaled" depth on land<sup>12-14</sup>. The criterion for excavation of soils, sand, and mud seems to be that the dynamic pressure of fluid flow must be large enough readily to interpenetrate the material. The lower limit to the pressure at which most of this interpenetration takes place seems to be determined by the dynamical pressure of the matter at the outer limit where cratering ceases and where the bulk of the material that is removed, or mixed, in the explosion resides. This dynamical pressure is simply the energy released by the explosive divided by the mass of material in motion with some modest correction for the inefficiency of energy transfer. For typical explosives, including nuclear, roughly half of the energy is left behind in heat, so that per gram of standard "high explosive",  $2 \times 10^{10}$  erg is distributed in 200 g of cratered material. This results in an energy density of  $10^8$  erg g<sup>-1</sup> of ejected matter, corresponding to 100 atm of dynamic pressure. The velocities at this point are subsonic, roughly  $10^4$  cm s<sup>-1</sup> or 1/20 the speed of sound in solid matter. Therefore, the mixing in cratering occurs well after the initial shock has passed.

## Explosive Equivalence of the Heat of Lava

The question of explosive mixing depends on whether the energy and pressure released in mixing is sufficient to cause the rapid further mixing of a still larger mass. The energy released when 1 cm<sup>3</sup> of lava at density 2.8 is cooled from a typical, 1,100° C to 100° C is approximately  $2.8 \times 10^3$  J—0.44 of which is released in the detonation of 1 cm<sup>3</sup> of explosive of

typical density of 1.6. Not all the heat of the lava is available to produce the  $PdV$  work that produces the typical cratering. If only a small amount of water comes in contact with a much larger mass of lava, the temperature, and hence pressure, of the steam will be very large, but only a small fraction of the heat will be removed from the lava, and the equivalent explosive mass will be small. At the opposite extreme, if a very much larger mass of water is mixed with the lava, a large fraction of lava heat will be invested in heating the water.

At temperatures above the critical temperature the latent heat of vapourization is continuous and a smaller fraction of the pressure energy, but at lower temperature, the latent heat is equal to or greater than the sensible heat in the steam. The optimum temperature for converting the lava heat to a form that can produce pressure times volume work will be somewhat above the critical temperature, where  $T_c \approx 370^\circ$  C. We envisage the initial mixing to proceed sufficiently rapidly that the pressure wave cannot reach the free surface of the water in the time of initial heating and hence the initial pressure should correspond to water at  $\rho \approx 1$  g cm<sup>-3</sup> and  $T \approx 400^\circ$  C. From Clark<sup>8</sup>,  $P \approx 6,000$  atm. If we include the compressibility of an equal mass of adjacent cold water, then  $\bar{\rho} \approx 0.9$  at  $\sim 3,500$  atm. The high pressure should lead to rapid further mixing of lava and high temperature water with subsequent expansion to the pressure of 100 atm corresponding to the cratering limit. The minimum heat requirement corresponds to maintaining the superheat of the steam at  $T = 400^\circ$  C. The enthalpy including the heat of vapourization and referred to 20° C is 720 calories g<sup>-1</sup> (ref. 15). The mass of water required per unit mass of lava can be calculated by equating the respective heats

$$m_1 H_v = m_2 C_L (T_L - T_v)$$

where  $m_1$  is the mass of water;  $H_v$  the enthalpy ( $\approx 720$  calories g<sup>-1</sup>) at  $T_v$ , the vapour temperature (400° C);  $m_2$  the mass of lava;  $C_L$  the heat capacity of lava ( $\approx 0.25$ ); and  $T_L$  the initial lava temperature ( $\sim 1,100^\circ$  C).

For these conditions,  $m_1/m_2 = 1/4$ . This is somewhat less than equal volume. Equal volume is roughly what is expected from the non-linear limit of growth (amplitude equals wavelength) of Taylor and Helmholtz instabilities when a final state of turbulence is reached. Taking into account the decreasing density of the water and steam, the ratio of  $m_1/m_2 \approx 1/4$  is reasonable, but it should be recognized that the mixing ratio should vary continuously from all lava to all water. We are concerned with estimates of the explosive effectiveness of the optimum ratio and ask even then whether autocatalytic mixing is likely.

## Explosive Energy Equivalent

The energy released by a high explosive is not all available for performing  $PdV$  work at pressures above the cratering pressure of 100 atm just as will be the case for the superheated steam discussed previously. But the combination products of high explosive contain a significant fraction of H<sub>2</sub>O as well as other molecules of N<sub>2</sub> and CO<sub>2</sub> that have a similar equation of state. Therefore, as a first estimate before detailed calculations can be performed, we compare the explosive effectiveness of high explosive and the water-lava mixture on an energy basis only.

The mass of lava that results in a quantity of steam at  $T_v$  equivalent to the energy content of 1 g of high explosive is

$$m_2 = \epsilon_{ex} / (C_L (T_L - T_v)) = 5.2 \text{ g}$$

where  $\epsilon_{ex}$  = specific energy released from high explosive =  $10^3$  calories g<sup>-1</sup>. The initial pressure of 4,000 to 6,000 atm is forty times larger than the criterion for the upper limits of cratering so that the additional mass of lava mixed with water per unit of equivalent high explosive should be the multiplication factor  $200/m_2 = 40$ . This factor is sufficiently greater than the value unity required for self-sustained mixing that only relatively modest or inefficient mixing is required to maintain the reac-



tion. The remaining question is whether the pressure of reaction can result in a configuration that ensures further mixing.

## Mixing

Mixing requires that the lava and water interpenetrate each other to an extent such that the particle size is small enough to exchange the heat content of water and lava within the time of dynamical mixing.

Actually, only a very small fraction (1/40 of the water and lava) needs to exchange heat energy in order to self-sustain the mixing and then, once mixing has initiated an "explosion", a further heat exchange can take place during the very much longer time of expansion of the whole mass in the atmosphere. But in order to understand the limiting lava particle size required to self-maintain mixing of the initial smaller fraction, we assume that once the pressure is relieved by expansion of the free surface of the water, the dynamical mixing described by the cratering processes would be terminated. Therefore, choosing the marginal condition at  $P=100$  atm, the time becomes  $t=h/U_{\text{exp}}$  where  $\rho_{\text{water}}U_{\text{exp}}^2/2=10^8$  dyne  $\text{cm}^{-2}$ , and where  $h$ =water depth;  $U_{\text{exp}}$  is the expansion velocity and so at  $h=30$  m water depth,  $t=0.2$  s. The upper limit to the lava particle size (or conversely water drop size) necessary to allow 50% heat transfer in this time is determined from equation (1) and results in the mixing condition that  $\delta \leq 4 \times 10^{-2}$  cm, or a diameter  $\leq 3\delta \approx 0.12$  cm.

This is probably not highly restrictive because:

(1) The ash ejected by the degassing or "venting" of the hot magma as it decompresses at the atmospheric surface of a caldera occurs just in this particle size range.

(2) Many photographs of the more modest lava-water eruptions show a "grey" ejecta of steam and lava made up of millimetre size particles of lava carried by a jet of steam.

(3) Ash rings of small "shards" of volcanic material are found surrounding submarine eruptions and are formed "as a result of secondary steam explosions from a volcanic eruption underwater"<sup>16</sup>.

(4) The material from large volcanic explosions is so finely divided that, in the case of the Krakatoa explosion, millions of tons of micron size particles remained suspended in the upper atmosphere for a year<sup>9</sup>.

The way in which the lava is so finely divided is harder to justify.

## Instabilities

Several well known instabilities lead to the mixing of two initially separated fluids that are set into relative motion. The first is the Rayleigh-Taylor instability that occurs when the two-fluid boundary is accelerated from the lighter fluid towards the heavier fluid. In this case, "tongues" of the two fluids interpenetrate each other. The next higher order of fluid instability, the Helmholtz instability, occurs on account of a differential shearing motion between the two fluids. In this case, waves are caused to grow to the point of breaking in a manner similar to ocean waves driven by the wind, but without gravity to hold the waves "down". Finally, a still more complicated phenomenon must occur at the water-lava interface due to the sudden chilling, solidification, and finally cracking or "spalling" of the fractured surface. Such a spalling occurs when molten glass is plunged into cold water. The first two fluid instabilities are complicated by the presence of the relatively high viscosity of the lava; the spalling instability depends upon the complexities of dynamical thermal stress and fracture propagation.

The Rayleigh-Taylor instability takes place at the interface between two fluids of differing density when the lighter fluid accelerates the heavier one. The growth rate  $\omega$  of a perturbation of wave number  $k=\pi/\lambda$  is:

$$\omega = [(\rho_2 - \rho_1)/(\rho_2 + \rho_1)gk]^{\frac{1}{2}} \quad (2)$$

where  $g$  is the acceleration.

The exponential growth (linear phase) is less important for our considerations than the non-linear phase of spikes of low density fluid interpenetrating the heavy fluid and bubbles conversely. Chandrasekhar<sup>17</sup> has shown that this phase of growth is limited by viscosity,  $\nu$ , such that the shortest wavelength to grow rapidly is:

$$\lambda_{\min} \approx 70^{2/3} g^{-1/3} \nu^{2/3} \quad (3)$$

We can derive this expression approximately by saying that growth is limited when the viscous diffusion ( $D=\nu/\rho$ ) is substituted in equation (1) for the diffusivity  $D$ , and the time,  $t$ , is taken as  $1/\omega$  from equation (2).

If we consider mixing when the disturbance is of the order of the wavelength of the interpenetrating finger or bubble, then

$$g \approx P/(\rho_L \lambda) \sim 10^9/\lambda \text{ cm s}^{-2} \quad (4)$$

where  $P \approx 3,500$  atm for the water before expansion. When we substitute this in equation (3)

$$\lambda_{\min} = 2 \times 10^{-3} \nu \text{ cm}$$

The viscosity of lava on the small scale and of  $1,100^\circ \text{C}$  ranges from  $10^3$  to  $10^5$  (ref. 18) so that wavelengths of 1 to 100 cm are indicated. Similarly in the case of molten metals, the viscosity is of the order 1 c.g.s. unit so that fine scale mixing readily occurs.

The Helmholtz instability depends on turbulence to give rise to the mixing once surface waves have begun to grow. Turbulence is limited by viscosity in a fashion that is expressed by the requirement that the Reynolds number of the smallest eddy, or by analogy the shortest wavelength, must be greater than 100. The expression for the minimum wavelength becomes

$$Rey = 100 = \lambda_{\min} U_s/\nu \quad (5)$$

where  $U_s$  is the velocity differential across the boundary. If we choose  $U_s$  to be its maximum value of  $10^4$  cm  $\text{s}^{-1}$ , and  $10^3 \leq \nu \leq 10^5$ , then  $\lambda_{\min}$  lies in the range 10 cm to 10 m, roughly the same as the Taylor instability case. Again, the same argument applies to molten metals. The combination of these two instabilities occurs when the interpenetrating tongues of the Taylor instability are eroded by the Helmholtz instability.

Daly<sup>19</sup> has shown by numerical methods the progressive non-linear growth of the combined Rayleigh-Taylor and Helmholtz instabilities and confirms the Chandrasekhar theory. The distance the average interface between lava and water moves in the time for the minimum wavelength instability to grow several  $e$  folds in amplitude becomes the time for the surface to translate a distance of several wavelengths, so that the interpenetration occurs at roughly the same rate as the translation of the interface. Therefore, even if we assume that the viscous "strength" of the lava is high, the unstable Taylor and Helmholtz mixing should take place in dimensions small compared with the thickness of the lava flow. To achieve the fine scale mixing necessary to give the particle size  $\lambda \sim 0.1$  cm, we must rely upon the "failure" of the partially crystalline structure of the cooling lava. The fine scale spalling analysis is uncertain; however, it is abundantly evident that such fine scale breakup of the lava occurs in any volcanic eruption even where the pressures are small compared with an atmosphere<sup>9</sup>.

## Geometry of Instability

Finally, we envisage a geometry where an initial pressure release at the lava-water interface is large enough to "crater" the crust and start a pressure wave radially along the interface. The pressure wave will travel faster in water than lava; hence, an annular high pressure region will push down on the lava, accelerating it downward and back toward the rarefaction at the original initiation point. The downward and radially inward acceleration of lava-water interface is both Taylor unstable as well as Helmholtz unstable from the velocity shear and should cause a major interpenetration and mixing of both fluids. Full mixing of optimum masses should give rise to a

forty-fold gain so that very much less stringent conditions are possible yet still give rise to an explosive release of energy.

### Minimum Size Perturbation

The minimum perturbation required possibly to initiate self-sustained mixing is of some interest. The initial disturbance should be sufficiently large that it "craters" a volume that penetrates the crust by a substantial fraction. If the crust is 1/3 m thick after a day, then the volume cratered should correspond to a hemisphere of radius roughly 2/3 m, or ~1 ton. If we assume a somewhat smaller cratering ratio of 100, because of the crustal strength, then the minimum mass of high explosive might be as low as 10 kg.

The properties of a natural phenomenon that could trigger such a mixing reaction become more restrictive. The rapid mixing or cratering condition requires a dynamic pressure of at least 100 atm. It is unlikely that when water "pours" into a collapsed caldera the overpressure created by the near steady state mixing can be much larger than twice the hydrostatic head. This then requires a depth of the caldera of at least 500 m or circumstances of "plugging" that create the same pressure.

Few volcanic explosions give rise to an atmospheric shock wave (which would require the sudden release of at least 1 atm pressure), so the sudden release of 100 atm is fortunately a rare phenomenon. Similarly, the difference in equivalent yield between the usual volcanic explosions and the events of Krakatoa and possibly Santorini is striking. The energy yield of most repetitive volcanic explosions is of the order of 10 to 100 ton high explosive equivalent, possibly as much as  $10^3$  ton for the undersea eruptions of the Myozin-syo volcano<sup>9</sup> south of Japan, whereas the Krakatoa equivalent yield was  $2 \times 10^8$  ton. Santorini may have been even larger. This suggests that two different mechanisms are involved.

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# Molecular Organization in the Sarcoplasmic Reticulum Membrane studied by X-ray Diffraction

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**Orientated preparations of rabbit sarcoplasmic reticulum vesicles give X-ray diffraction maxima to a resolution of 16 Å. The calculated electron density profile shows that protein is distributed very asymmetrically in the membrane.**

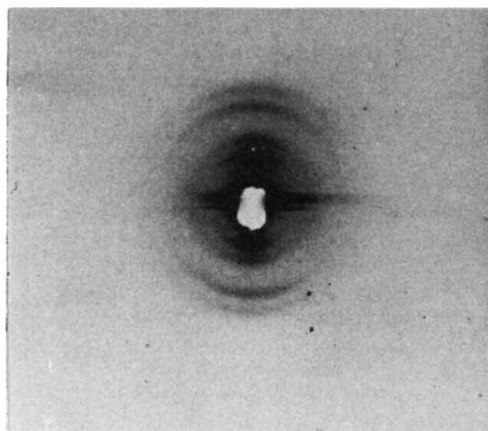
THE membrane system of the sarcoplasmic reticulum (SR) regulates the contractile cycle of muscle filaments by the

uptake and release of calcium ions<sup>1-3</sup>. Closed vesicles derived from the SR have been isolated in good yield from muscle homogenates and shown to concentrate  $\text{Ca}^{2+}$ , using ATP as an energy source, in conditions similar to those prevailing *in vivo*<sup>4</sup>. The vesicles contain protein and lipid, in a ratio of about 65:35 (ref. 5). Several groups have shown that a major protein component, approximately 100,000 in molecular weight, accounts for a fraction of the total protein variously estimated as 30 to 60%<sup>6-8</sup>. The isolated protein possesses  $\text{Ca}^{2+}$ -stimulated ATPase activity<sup>6,9</sup>.

Electron microscopy of SR membranes reveals that the preparations contain vesicles ranging in diameter from 500 to several thousand angstroms<sup>10</sup>. Vesicles negatively stained with uranyl acetate and phosphotungstic acid (PTA) appear to show an outer layer of 30-40 Å "spikes" or "particles" project-

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**Fig. 1** X-ray diffraction photograph of orientated SR membranes. The second, third, fourth, sixth and seventh orders of a 234 Å repeat are visible. The orders 9, 11 and 13 can be seen on the original film. Equatorially orientated diffraction appears at 55 Å (just outside the position of the fourth meridional order). The photograph was taken with a mirror-monochromator point-focusing camera, used with an Elliott rotating-anode X-ray generator (focal spot 100 μm). The horizontal streaks are parasitic scatter from the quartz monochromator.

ing from the membrane surface, and freeze-etched preparations have somewhat larger (80 Å) particles in the plane of cleavage<sup>11</sup>. At least part of the ATPase molecule must be accessible from the exterior of the vesicle, as shown by ferritin active-site label experiments<sup>10</sup>. To define in greater detail the geometry of the SR membrane, we have prepared orientated specimens for X-ray diffraction. Our results show a distinct asymmetry in the organization of the SR membrane.

## Orientated SR Vesicles

SR vesicles were prepared from rabbit skeletal muscle by standard methods<sup>12,13</sup>. We introduced an additional purification just before preparation of the X-ray specimen by centrifuging through a discontinuous sucrose gradient (Spinco SW65 rotor, 25,000 r.p.m., 2 h). The largest fraction was consistently found in a band between sucrose solutions of density 1.11 g cm<sup>-3</sup> and 1.13 g cm<sup>-3</sup>, and this fraction, dialysed against 0.02 M Tris-maleate, pH 7.2, 0.1 M KCl, 5% glycerol, was used for X-ray studies. Unfractionated vesicles gave identical diffraction patterns but with somewhat broader reflexions. Orientated specimens were obtained by centrifuging approximately 5 mg of SR (Spinco An-D rotor, 56,000 r.p.m., 12 h, 4°C) onto a mica strip, 1 mm × 12 mm, inserted into a 'Teflon' adapter at the bottom of a Beckman Model E analytical centrifuge cell. States of different water content were obtained by air-drying the 1 mm thick pellet in the cold for periods of up to 2 h. Coleman *et al.*<sup>14</sup> used similar techniques for orientating SR and other membrane preparations.

The specimens were cooled during X-ray exposure, and no change of spacing was observed for periods of up to 4 d. Enzymatic activity of the preparation survived the orientation, partial drying, and X-ray exposure, as shown by the data in Table 1. Ca-stimulated ATPase assays showed 80 to 100% specific activity in re-homogenized samples that had been dried to give X-ray spacings between 190 and 265 Å. Moreover, Ca<sup>2+</sup>-uptake measurements indicated that a significant fraction of the re-homogenized vesicles were closed and capable of sequestering Ca<sup>2+</sup>.

An X-ray diffraction photograph of the SR membranes (Fig. 1) shows along the meridian, orders of a single repeat whose spacing varies with the water content of the membranes. The diffraction generally extends to a resolution of 16 to 20 Å, depending on the state of the specimen. In many photographs we can detect traces of a meridional reflexion at about 60 Å, which becomes much stronger in very dry or maltreated

samples. We attribute this line to an independent lipid phase, presumably from disrupted membranes. There are also rather diffuse equatorially orientated reflexions at 55 Å and in the 10 Å region. Our photographs are thus significantly better than those of Coleman *et al.*<sup>14</sup> who obtained only a few orders from centrifuged, dried SR membranes.

**Table 1** ATPase Activity and Rate of Ca<sup>2+</sup> Uptake in SR Membranes after X-ray Exposure

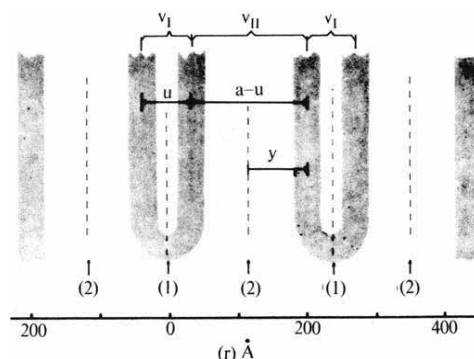
|           | 190 Å | X-ray spacing<br>240 Å | 260 Å |
|-----------|-------|------------------------|-------|
| ATPase    | 0.8   | 0.8                    | 1.0   |
| Ca-uptake | 0.3   | 0.5                    | 0.7   |

Activities are normalized to the measured activity (taken as 1.0) of an equivalent weight of fresh SR membranes, before preparation for X-ray experiment.

See ref. 8 for methods.

## Spacing and Water Content

The meridional spacing varies continuously from 270 Å for a freshly packed specimen not subjected to drying (50% water by weight) to 170 Å for a rather dry specimen (25% water). We observed altogether more than fifteen different spacings. In one case we obtained eight different spacings by successive dryings of a single specimen, an experiment which permitted us to follow all the reflexions and to ensure that no reflexions from lipid phases were included in our measurements.



**Fig. 2** Schematic diagram of packed vesicles. Considered in one-dimensional projection along the line perpendicular to the membrane planes, the distribution of matter has centres of symmetry between collapsed vesicles (2) and at the vesicle interior (1). The unit cell spacing is denoted by  $a$  and the two inter-membrane distances by  $u$  and  $a-u$ . The internal and external volumes  $v_I$  and  $v_{II}$  are shown to correspond to  $u$  and  $a-u$ , respectively. Evidence for this assignment is discussed in the text under the heading "Electron Density Profile." The scale at the bottom of the figure corresponds to a spacing  $a=234$  Å.

The 170 to 270 Å repeat distance contains two apposed membranes; thus the line group of our one-dimensional unit cell has centres of symmetry. The shrinkage accompanying removal of water can in principle occur at either interface or at both—that is, at the vesicle interior or at the inter-vesicle surface (Fig. 2). Comparison of one-dimensional Patterson functions at different spacings indicates that over particular ranges of hydration, the shrinkage occurs in a relatively simple manner. The Patterson corresponding to a repeat distance  $a=234$  Å appears in Fig. 3a. The peak at 37 Å remains essentially constant with spacing over the entire range; we attribute it to the inter-polar group vector (compare ref. 15). The strongest feature is the maximum at  $u=68$  Å. If we ascribe this to a vector,  $u$ , between two membranes in a unit cell, then  $a-u=166$  Å is the other inter-membrane distance (compare

Fig. 2). Note that the separations  $u$  and  $a-u$  are very unequal, suggesting a difference in the inside-outside distribution of mass. In Fig. 2, we have shown  $u$  as the vector across the interior of a single collapsed vesicle (volume  $v_I$ ) and  $a-u$  as the vector between vesicles (volume  $v_{II}$ ). This assignment is at present arbitrary, but we indeed conclude below that there is more protein on the outer vesicle surface than on the inside and that the longer vector ( $a-u$ ) must extend across the interface of apposed vesicles.

The dependence of the distances  $u$  and  $a-u$  on the spacing  $a$  is shown in Fig. 3b. In the "wet" range  $220 < a < 270$ ,  $u$  varies more rapidly than  $a-u$ . In the "dry" range  $170 < a < 220$ ,  $a-u$  changes, while  $u$  remains constant. Within each range it should therefore be possible to trace a continuous transform by scaling diffracted amplitudes from specimens of different spacing. In the "dry" range this approach will depend on the assumption that no significant structural changes occur within the shrinking volume  $v_{II}$ , and in the "wet" range by the further assumption that the slower variation of  $v_{II}$  may be ignored in comparison with the shrinkage of  $v_I$ . We can evaluate these assumptions by analysing the continuous transforms themselves.

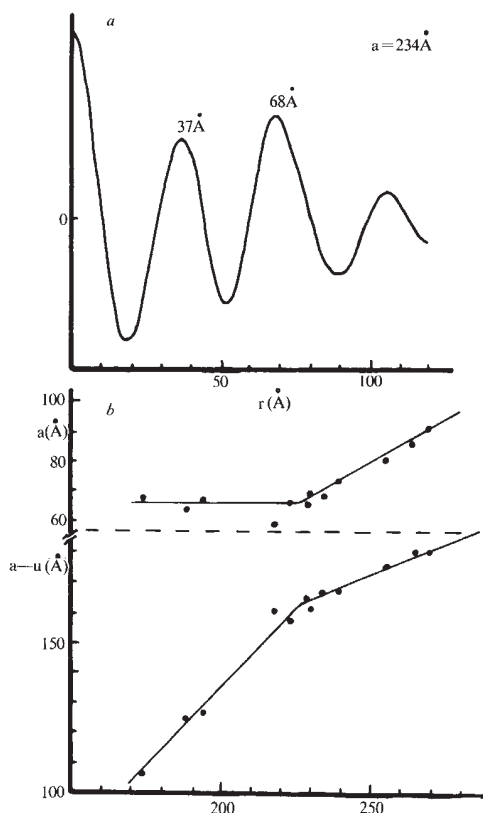


Fig. 3 *a*, One-dimensional Patterson function, calculated using intensities measured from the photograph in Fig. 1 ( $a = 234$  Å). The photograph was photometered with a small beam along the meridian, and the  $F^2(h)$  were obtained from the area under each peak by applying a Lorentz factor proportional to  $h^2$  (as for a powder pattern). The internal and external volumes  $V_I$  and  $V_{II}$  are shown to correspond to  $u$  and  $a-u$  respectively. *b*, Dependence of  $u$  and  $a-u$  on total spacing,  $a$ . See Fig. 2 for definitions.

The transforms are shown in Fig. 4. They consist of a series of alternating peaks and nodes. Within each peak, the signs of all terms must be the same. We can invoke the principle of minimum wavelength<sup>17</sup> and conclude that the signs of the first three peaks of the dry transform and of the first six of the wet transform must alternate. The phases are thus determined to within an overall sign ambiguity to a resolution of about 30 Å. We have calculated Fourier syntheses to resolve this ambiguity:

only one of the two sign sequences gives a plausible profile (Fig. 5). It is not possible to continue the phasing in this manner, however, due to the broad region of low amplitude in the continuous transform near  $0.0037$  Å<sup>-1</sup>. That is, the signs of the fourth and fifth maxima in the dry transform or of the eighth, ninth and tenth in the wet transform are undetermined with respect to the low resolution terms. Within the region  $0.04$  Å<sup>-1</sup>  $< R < 0.06$  Å<sup>-1</sup> the minimum wavelength argument again applies: only an overall ambiguity of high-resolution signs with respect to low resolution signs remains unresolved. With available data, we are unable to make a choice between the two alternatives.

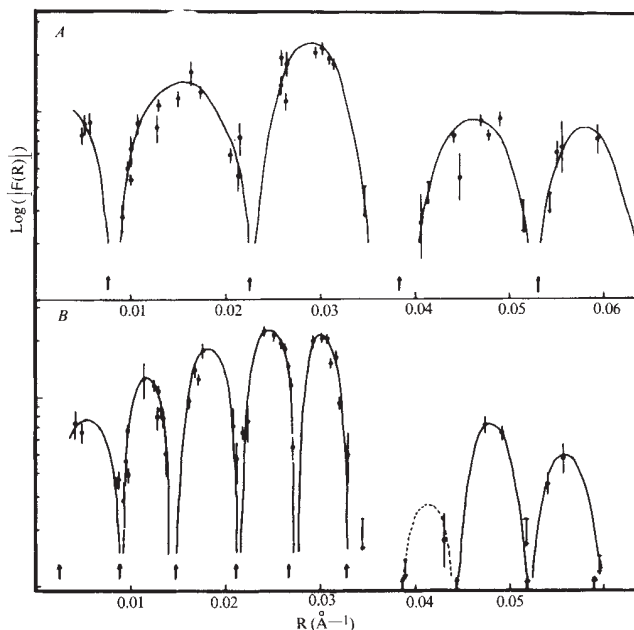


Fig. 4 Continuous transforms for SR membranes in wet and dry hydration ranges, derived by combining the corrected diffraction amplitudes corresponding to various spacings  $a$ . The data for different spacings were scaled by normalizing the sum  $\sum h^2 I(h)$  for all reflexions between 0 and  $(30 \text{ Å})^{-1}$ .

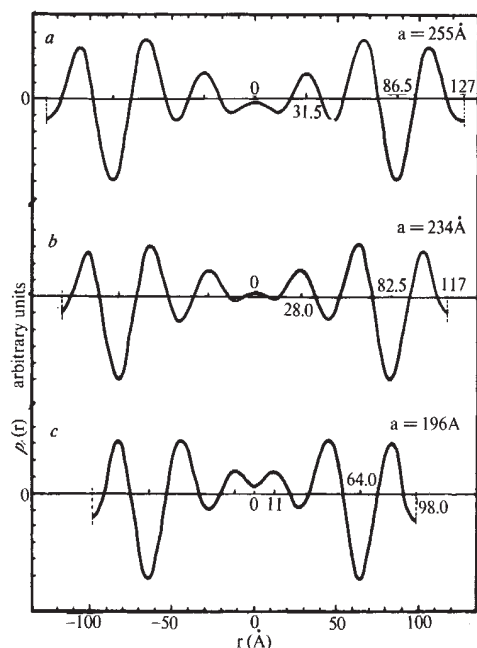
*A*, "Dry" transform:  $190 \leq a \leq 234$ . Arrows indicate nodes of  $\cos 2\pi(34)R$ ; *B*, "wet" transform:  $220 \leq a \leq 240$ . Arrows indicate nodes of  $\cos 2\pi(83)R$ . We can understand the spacing of the nodes for each continuous transform by considering that the principal contrast in the electron-density variation across the membrane comes from the essentially centrosymmetric profile of a lipid bilayer, as in the treatment of the myelin structure by Caspar and Kirschner<sup>16</sup>. We do not need to introduce the assumption of a centrosymmetric bilayer into our analysis. We describe the continuous transforms in these terms here simply to make clearer the nature of their variation. Let  $f(R)$  be the transform of this profile referred to an origin at its centre and let  $y$  and  $(a-y)$  be the positions of the membrane centres in the unit cell (compare Fig. 2). Then the transform of the two-membrane structure, referred to the proper unit-cell origin, is  $2f(R) \cos 2\pi yR$ . In the "wet" range, it is appropriate to use centre of symmetry 2 (Fig. 2) as the origin, since we take  $v_{II}$  to be approximately constant. Then  $y = 1/2 (a-u) = 83$  Å and the nodes of  $\cos 2\pi yR$  are rather closely spaced, coinciding well with the nodes of the "wet" transform. In the "dry" range,  $v_I$  is constant, and choosing centre of symmetry 1 (Fig. 2) as the origin, we obtain  $y = 1/2 u = 34$  Å. Again, the nodes of the observed transform coincide approximately with the zeroes of  $\cos 2\pi yR$ .

## Electron Density Profile

Figure 5 shows the distribution of electron density across the SR membrane, calculated from the unambiguously-phased low-resolution reflexions. The profiles for three different hydration states are given. Their similarity is consistent with the arguments presented above, that no major structural changes occur on mild dehydration, and it is indeed this similarity that has



been the basis of our approach to determining the structure. An analogous method has been used in X-ray studies of myelin<sup>16</sup>, and the comparison of different lattices containing identical structures is also a useful technique in protein crystallography<sup>18</sup>.



**Fig. 5** Electron density distribution across the SR membrane. Density in arbitrary units is plotted as a function of the distance,  $r$ , across the membrane. The three profiles were computed to a resolution of 30 Å using the unambiguously phased reflexions, with signs indicated below.  $a$ ,  $a=255$  Å:  $+0-+0-++$ ;  $b$ ,  $a=234$  Å:  $+- - + + - - +$ ;  $c$ ,  $a=196$  Å:  $+- - + + - -$ .

An asymmetrically placed minimum is the most prominent feature of the SR electron density profile. It indicates a bilayer organization of lipid in the membrane. The maxima to either side are spaced about 40 Å from each other: they show the position of lipid polar groups and associated protein (compare the low resolution profile of myelin). A density maximum appears at about 50–55 Å from the bilayer centre. Its consistent appearance in all three profiles indicates that it is a significant feature, and not a Fourier cut-off effect. It must represent protein that projects from one side of the membrane, and we can probably identify it with the projections seen in electron micrographs of negatively stained vesicles<sup>11</sup>. This interpretation determines the inside and outside of the vesicle in our profiles. It is presumably the projecting structure that gives rise to the large asymmetry in  $u$  and  $a-u$  (Fig. 3b). The minimum inter-membrane distance observed on the external side with the projecting density maximum is about 110 Å, whereas the membranes can approach on the internal side to a distance of 66 Å.

## SR Membrane Organization

We have demonstrated that there is in the SR membrane a strongly asymmetric distribution of protein about a lipid bilayer. The minimum in the electron density profile does not, however, imply that this bilayer be continuous. Indeed, the composition of the SR requires that some protein penetrate—or dip into—the lipid. We can see this by calculating the length fraction of the one-dimensional unit cell occupied by lipid. The data in Table 2 show that one lipid bilayer has an “equivalent thickness” of about 26 Å. Since the distance between polar groups estimated from the electron density profile is about 40 Å,

the calculation implies that 30–40% of the volume in the inter-polar group region contains either protein or water. A model with either partial or complete penetration by the ATPase protein is clearly the most plausible. The projections and the corresponding peak in the density profile could then represent either a tail of the ATPase molecule itself or a further protein component. The magnitude of the electron density maximum suggests that it includes about 20% of the total membrane protein. The reconstituted membranous structures obtained from purified ATPase show no projections<sup>19</sup>, but the organization of the reconstituted structure cannot at this stage be assumed identical to the native.

**Table 2** Length Fractions of the Repeat Distance Occupied by Lipid, Protein and Water for Wet and Dry Orientated SR Vesicles

| Spacing (a) | Water content<br>( $\pm 5\%$ ) | Lipid | Length fraction<br>Protein | Water |
|-------------|--------------------------------|-------|----------------------------|-------|
| 270 Å       | 50 %                           | 0.19  | 0.26                       | 0.55  |
| 170         | 25 %                           | 0.30  | 0.41                       | 0.29  |

The calculations are based on a protein : lipid ratio of 65 : 35 and on the water content determined by weighing a sample before and after drying over  $P_2O_5$ . We assumed mean densities of 1.0 and 1.37 for lipid and protein respectively.

Do our results provide constraints for describing the mechanism of  $Ca^{2+}$  transport in the SR? The fact that we can record discrete meridional reflexions out to 15 Å resolution indicates that there is a relatively high degree of order across the membrane. Moreover, the asymmetry of the structure is its most striking feature. The ATPase molecules must therefore adopt quite uniform orientations and positions with respect to the lipid bilayer. Isolation of the ATPase and restoration of activity by added lipid make it reasonable to assume that in the membrane this protein and associated lipid represent the entire functional transport unit<sup>9,19,20</sup>. Since major translations or reorientations of the large ATPase proteins would require significant disruption of the asymmetric organization, our results favour a description of  $Ca^{2+}$  transport in which the transport system functions with at most relatively modest conformation changes.

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# LETTERS TO NATURE

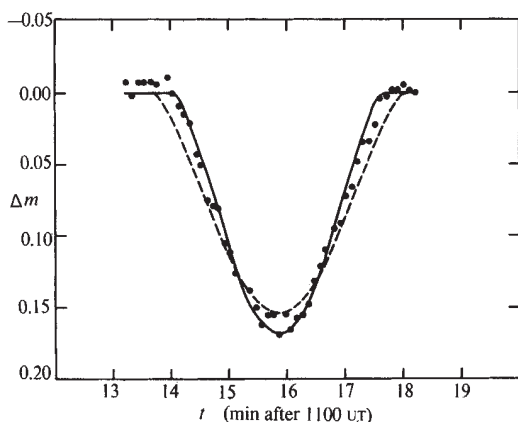
## PHYSICAL SCIENCES

### Polar Cap on Europa

WE report here photoelectric observations of two occultations of Jupiter's satellite Europa (JII) by the satellite Io (JI). The reductions in the intensity during these events exceeds predicted values (K. A., unpublished) by 80% and 55% for the nights of June 17, 1973, and June 24, 1973, respectively. The durations and absolute times of occurrence are in close agreement with the predictions, indicating that errors in the orbital parameters or in the satellite diameters are not introducing large errors into the predicted light curve. We therefore conclude that Europa has a bright polar cap extending from the north pole to latitude  $\sim 30^\circ$ .

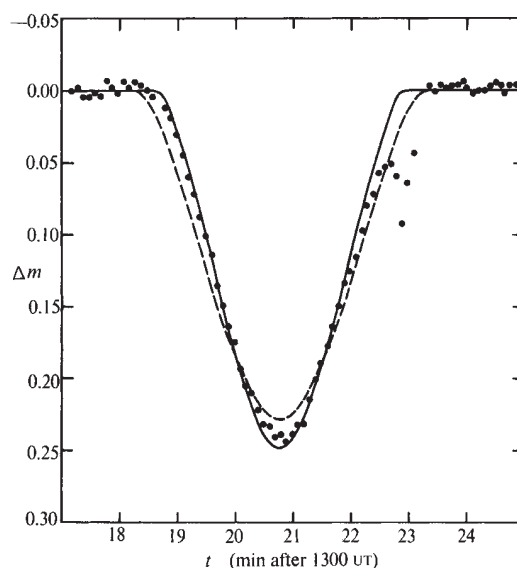
The observations were made with a 61-cm telescope and a photon counting single beam photometer at the Mauna Kea Observatory. A cooled S20 photomultiplier tube and a Stromgren  $y$  filter ( $\lambda_c = 5500 \text{ \AA}$ ) were employed. The sky signal was about 1% of the satellite signal for the 22 arc s aperture due to the large radial distance of the satellites (4.5 Jupiter radii) and the use of the freshly aluminized primary mirror. Sky readings were made  $\sim 13$  arc s north and south of the satellites several times before and after each event and once during each event. Except for one instance of severe wind-loading on the telescope near the end of the event of June 24, variations in the sky signals were insignificant.

The observations of the event on June 17, 1973, are shown in Fig. 1. Each filled circle represents a 4 s integration. The predicted eclipse depth was 0.09 mag, based on satellite radii of 1,829.5 km and 1,550 km, and geometric albedoes of 0.80 and 0.72 for Io and Europa, respectively. The disks were assumed to have a uniform surface brightness. Because the observed



**Fig. 1** Light curve for Europa occultation by Io on June 17, 1973, at 1116 UT. The predicted depth was 0.09 mag. Each filled circle represents a four second integration. —, Light curve which results from the nominal orbital parameters but with the average geometric albedo of the occulted portion of Europa equal to 1.30 while the total geometric albedo of Europa is 0.72. ---, Light curve which results from a modified orbit with the separation of the satellites decreased by 400 km but with Europa having a uniform geometric albedo of 0.72.

eclipse depth is 0.17 mag, we have attempted to reproduce the light curve by decreasing the impact parameter, thereby increasing the fraction of Europa which is occulted. The dashed curve in Fig. 1 shows the predicted light curve if the satellite separation at mid-occultation is decreased by 400 km from the nominal orbit. Although the predicted depth of the eclipse (0.15 mag) is nearly equal to that which was observed, the predicted duration (260 s) is significantly longer than the observed duration ( $210 \pm 10$  s). The solid curve in Fig. 1 is the predicted light curve if the occulted portions of Europa have a geometric albedo of 1.30, while the average albedo remains 0.72. The duration (200 s), depth (0.17 mag), and the shape of the predicted curve closely match the observations.



**Fig. 2** Light curve for Europa occultation by Io on June 24, 1973, at 1321 UT. The predicted depth was 0.15 mag. The symbols are as in Fig. 1 except that — represents the light curve for the nominal orbits and an average geometric albedo of the occulted portion of Europa of 1.12.

In Fig. 2 we show the observations of June 24, 1973, together with predicted light curves for the nominal orbital parameters with the satellite separation decreased by 400 km (dashed line), and for the nominal orbital parameters with the albedo of the occulted portion of Europa equal to 1.12 (solid line). As before the average albedo of Europa remains 0.72. The observations are marred by an excursion towards Jupiter caused by wind loading on the telescope, and it is difficult to assess accurately the duration of the event. The first half of the event required  $120 \pm 10$  s, from which we estimate  $240 \pm 20$  s for the total event. The nominal orbit yields a duration of 240 s while the modified orbit yields a duration of 290 s. The observed semi-duration is then in better agreement with the nominal orbit than with the modified orbit. It is clear that the observed depth, 0.24 mag, exceeds the nominal depth, 0.15 mag, and that it may be explained by a bright polar cap



with an average geometric albedo of 1.12. The lower albedo of the larger occulted area on June 24 implies that the additional area occulted in this event is a mixture of high and low albedo material.

The existence of bright polar regions on Europa was suggested by the drawings of Lyot<sup>1</sup>, who found Europa to be markedly different from the other Galilean satellites. The present observations confirm the correct nature of his drawings and indicate that very high contrasts are involved. Pilcher *et al.*<sup>2</sup> and Fink *et al.*<sup>3</sup> have shown that between 50% and 100% of the light from Europa is reflected from water ice. The exact boundaries of the polar cap cannot be specified from the present data alone, but we may estimate that it covers about 30% to 40% of the surface. With a similar polar cap in the southern hemisphere between 60% and 80% of the surface would be snow covered, and the remaining surface must necessarily consist of very low albedo ( $0.0 < P < 0.1$ ) material such as that found by Murphy *et al.*<sup>4</sup> on the dark side of Iapetus. It is likely, however, that at least some of the discrepancy in the eclipse depths will be due to geometric factors. The extreme albedo values suggested there would then be moderated, but the concept of polar ice caps covering a low albedo surface is unlikely to change.

Complete mapping of this same face of Europa is possible if high quality photoelectric observations are obtained of the entire series of occultations of Europa by Io. All observers are urged to observe every event visible from their location and to forward their observations to the data bank: c/o Dr R. L. Millis at the Lowell Observatory, Flagstaff, Arizona, United States. Detailed prediction of the events are available from several sources<sup>5-7</sup>.

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## Praesepe and the Solar Neutrino Problem

Sagan and Young<sup>1</sup> have suggested that the solar neutrino problem and the apparent presence of river channels on Mars may both be resolved by variations of 10% to 20% in the solar luminosity, and they have cited the observed width of the main sequence in Praesepe as evidence that such luminosity

excursions have occurred in other solar-type stars. But the main sequence width has previously been interpreted<sup>2,3</sup> as due to the observed spread in rotational velocity of the stars. Are these interpretations mutually exclusive or could they be compatible?

Using the data of Johnson<sup>4</sup>, Sagan and Young found that the standard deviation for the main sequence stars in Praesepe with  $0.3 \leq B-V \leq 0.6$  is about  $\pm 0.1$  mag. I have obtained a similar, though slightly larger, result using more recent data<sup>5</sup>. Stars of later type show an appreciably smaller scatter. This result agrees qualitatively with the known facts that stars of earlier spectral type rotate faster on average<sup>6</sup> and that rotation moves a star upwards and to the right in a colour-magnitude diagram<sup>7</sup>. Thus the sample of earlier spectral type contains stars with a greater spread of rotation speeds and so a greater spread in magnitude.

Theoretical considerations<sup>7</sup> suggest that the vertical spread of the main sequence in magnitude,  $\Delta M_V$ , is approximately related to the rotation speed  $v$  by the relation

$$\Delta M_V = \chi v^2$$

where  $\chi$  is roughly constant along the main sequence for stars with a given angular momentum distribution. In units of  $10^{-5}$  mag  $(\text{km s}^{-1})^{-2}$ ,  $\chi$  has the value 0.4 for uniformly rotating stars. Using the data of Dickens *et al.*<sup>3,5</sup>, the root mean square velocity of stars in Praesepe with  $0.3 \leq B-V \leq 0.6$  is about  $100 \text{ km s}^{-1}$  and so the expected standard deviation in magnitude if the stars are rotating uniformly is  $\pm 0.04$  mag—less than half the observed spread. The previous interpretation of this result<sup>2,3,8</sup> has been that the stars must be rotating non-uniformly, though the data is not accurate enough to favour any particular rotation law<sup>3</sup>. On that interpretation, there is no need to invoke any further broadening mechanism, and the main sequence dispersion in Praesepe gives no support to the hypothesis of Sagan and Young.

But one could also argue that the stars are rotating uniformly—as seems to be the case for stars with  $B-V < 0.3$  (ref. 9)—and that rotation contributes only  $\pm 0.04$  mag to the observed spread. If the luminosity excursions required by Sagan and Young were only slightly less than they suggest, and gave a standard deviation of  $\pm 0.09$  mag, the combined standard deviation would be the observed  $\pm 0.1$  mag. An argument against this interpretation is that, as noted by Sagan and Young<sup>1</sup>, the stars in Praesepe are distributed approximately uniformly within the main sequence band. This is consistent with the known, rather flat distribution of rotation velocities but is surprising if, as has been suggested<sup>10</sup>, the <sup>3</sup>He mixing episodes last for a time short compared with the time between mixings.

I conclude that, although the evidence from Praesepe does not necessarily contradict the hypothesis of Sagan and Young, neither does it positively support it and it is possible to explain the entire observed width of the main sequence in a much less exotic way. Evidence for episodic mixing in solar-type stars must be sought elsewhere.

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## Two Probable Astroblemes in Brazil

MORE than forty structures have now been identified as astroblemes, ancient geological scars produced by the impacts of large cosmic bodies on the Earth<sup>1</sup>. We report here some preliminary results on the study of two new astroblemes in Brazil, the Araguainha Dome and the Serra da Canghala structure. Although relatively young meteorite impact craters have been identified in Argentina<sup>2</sup> and Chile<sup>3</sup>, these two Brazilian structures are the first ancient astroblemes to be reported from South America. The impact origin of the Araguainha Dome has been established by the discovery of diagnostic shock-metamorphic effects<sup>4</sup> in its rocks. No shock effects have yet been identified in rocks from the Serra da Canghala structure.

The Araguainha Dome, centred at 16° 46' S, 52° 59' W, is located near the town of Araguainha in the state of Mato Grosso, Brazil. The structure has been regarded as an alkaline intrusive, of which there are many examples in Brazil. It consists of a central uplift about 10 km across, in which cataclastically deformed Devonian Furnas Sandstone is exposed. Around this uplift is an annular ring syncline in which Permo-Carboniferous sediments, chiefly sandstones, are exposed in a series of downdropped grabens. The diameter of the structure estimated from ground studies is about 20 km. But a photograph of the structure taken from the ERTS-1 satellite (ERTS-1 photograph 1089-13005; October 20, 1972) shows that the circular pattern extends across a diameter of about 40 km (Fig. 1). If this pattern represents the true size of the structure, then the Araguainha Dome is one of the larger known astroblemes.

The central part of the central uplift is occupied by an unusual igneous breccia which contains inclusions of buried quartzofeldspathic Precambrian crystalline rocks in an igneous matrix. Thin sections of this breccia and the inclusions, made available to us by officials of *Petróleo Brasileiro SA* (Petrobras), contain a wide variety of distinctive shock-metamorphic effects, which are diagnostic of the intense shock pressures ( $\geq 100$  kbar) produced by meteorite impact. The inclusions show extensive development of planar features (shock lamellae) in quartz, often as many as six to eight distinct sets of lamellae per grain, with orientations corresponding to the distinctive  $\{10\bar{1}3\}$ ,  $\{10\bar{1}2\}$ ,  $\{11\bar{2}2\}$ , and  $\{11\bar{2}1\}$  planes<sup>5,6</sup>. The igneous matrix of the breccia



Fig. 2 Aerial photograph of Serra da Canghala structure, showing high wall of uplifted Poti Sandstone around the central uplift. Part of the downdropped ring syncline is in the foreground. White streak at left is an aircraft landing strip.

exhibits a heterogeneous texture and contains numerous shocked and partly melted rock and mineral inclusions. The breccia from the Araguainha Dome is identical in appearance to impact produced breccias observed at other astroblemes.

The Serra da Canghala structure ("Pack-Saddle Mountain") is located in the remote northern corner of the state of Goiaz (8° 05' S, 46° 52' W) in north-eastern Brazil, on the southern limb of the Amazon basin. The structure (diameter 12 km) has a damped-wave form with a central uplift of diameter 5 km surrounded by a downdropped annulus at least 3.5 km across. Serra da Canghala is a remarkably impressive feature of the region. In the central uplift, a circular wall, 250 m high, of steeply dipping Poti (Carboniferous) Sandstone surrounds a central basin of uplifted Longa (Devonian) Shale (Fig. 2). The central part of the structure thus has a crater-like appearance resulting from the preferential erosion of the Longa Shale by streams which flow through a breach in the Poti Sandstone wall.

In 1971 the Company for Research on Mineral Resources (CRPM) drilled three diamond drill holes, each 200 m deep, within the centre of the structure to test the possibility that the structure might be a kimberlite intrusion. No igneous rocks were found, and the drilling encountered only highly deformed and steeply dipping Longa Shale. The drilling results and the surface outcrop pattern indicate that the central uplift becomes progressively more disturbed towards the centre of the structure and displays the forms of an eviscerated dome. The intensely disturbed central area, the down thrown peripheral annulus, and the ratio of the central uplift diameter to total diameter are consistent with the structural features found in astroblemes. The overall structural pattern and geomorphology are strongly similar to those at the Gosses Bluff astrobleme in central Australia<sup>7,8</sup>.

The rock units underlying the Serra da Canghala structure down to the Precambrian basement are entirely clastic and do not contain significant carbonate or salt units; the structure cannot be a salt diapir. Igneous rocks are entirely absent from the structure, and large cross-cutting sandstone dikes are the only anomalous features.

One of us (R. S. D.) made a three-day reconnaissance and collection at the Serra da Canghala structure in October 1972. Shatter coning, a useful field criterion for meteorite impact<sup>9</sup>, was not observed. The rocks collected were almost entirely fine-grained siltstones which did not show any shock-metamorphic effects. One specimen of coarser grained quartzite, collected as a cobble in a conglomeratic unit of the Poti Sandstone in the central part of the structure, showed small areas characterized by unusual fracturing of the quartz grains. Similar fracturing in quartz has been reported in mildly-shocked rocks from known meteorite impact craters<sup>10,11</sup>, but the features observed in the Poti Sandstone are not distinctive enough to be considered diagnostic for meteorite impact. At present, the structural deformation in the Serra da Canghala structure, combined with the apparent

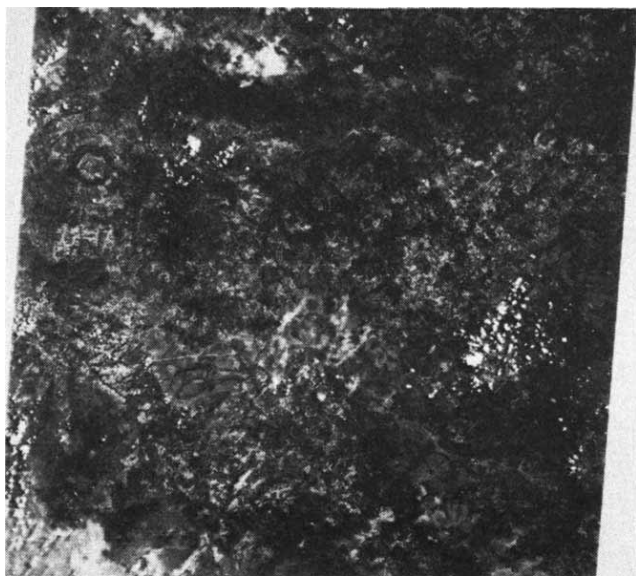


Fig. 1 ERTS-1 photograph of the Araguainha Dome. The structure appears at upper left as a dark inner ring and an outer circular pattern about 40 km across. NASA photograph, courtesy N. M. Short.



absence of igneous activity, favours the theory that the structure is an astrobleme. But definite proof will require the identification of unquestionable shock-metamorphic effects in its rocks.

We thank the Company for Research on Mineral Resources (CRPM) for making possible the field work at Serra da Canghala and for providing the assistance of J.-M. Coehlo in the field. M. Antonio M. de Oliveira of Petróleo Brasileiro SA (Petrobras) provided the thin sections and information on the Araguinha Dome. Petrographic studies were carried out at NASA, Goddard Space Flight Center, courtesy of C. C. Schnetzler. N. M. Short, also of NASA, Goddard, provided information and ERTS photographs of the Araguinha Dome. Partial support for R. S. D.'s travel to South America was provided by the Barringer Crater Co., Philadelphia, Pennsylvania.

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## Evidence of Features in Atmospheric Spectra at around 8 cm<sup>-1</sup> of Probable Solar Origin

OUR studies of emission spectra of the stratosphere in the frequency range 30 to 200 cm<sup>-1</sup> from a Caravelle<sup>1,2</sup> have revealed lines due to the principal atmospheric components H<sub>2</sub>O, O<sub>2</sub> and O<sub>3</sub>.

The flights were made during night and day and the spectra were then compared in order to identify the possible existence of photoreactions or other phenomena induced by the Sun. An unexpected feature always appeared in the day spectra around 8 cm<sup>-1</sup>, but never in the night spectra (Fig. 1). This could not be due to some instrumental artefact, and effectively laboratory spectra do not show any anomalous peak in this spectral region.

Outside the frequency range 30 to 200 cm<sup>-1</sup> the spectral response is negligible, so the signal we detected cannot reasonably be attributed to any atmospheric spectral feature. On the contrary, because Fourier transform techniques are used, time fluctuations of the atmospheric background

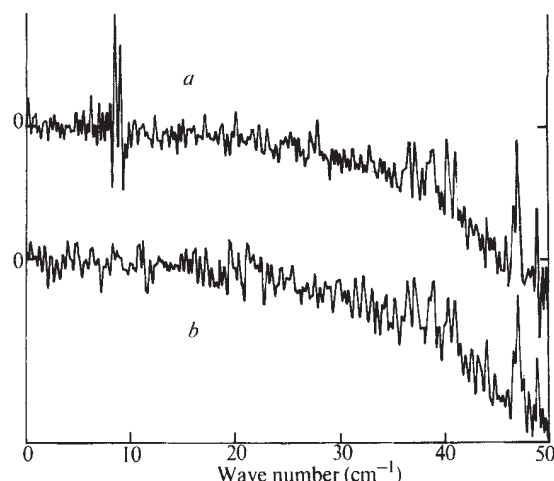


Fig. 1 Examples of two different emission spectra of the sky obtained at 12.5 km. The upper spectrum (a) was recorded during a day flight and shows a large feature around 8 cm<sup>-1</sup>. The lower spectrum (b) was recorded in the night flight subsequent to that in which spectrum a was recorded and does not show any evidence of similar peaks.

may cause the kind of features that we observe. Actually, the interferogram contains information not only about the spectral distribution of the energy but also about its time fluctuations. In our case each step is either 20 μm in optical path difference or 0.850 s in time. We have carried out a power analysis of the signal to determine its time characteristics. The interferograms have been convolved with a low pass filter reducing, in the spectral band of interest, the contribution of the noise coming from the remaining part of the frequency band. Three power spectra of consecutive interferograms are shown in Fig. 2. The measurements were done at an altitude of 11.6 km with a spectral resolution of 0.18 cm<sup>-1</sup>. Strong peaks appear at 20.31 and 21.25 mHz (a), 19.65 mHz (b), 17.4 and 19.65 mHz (c). The frequency resolution being 0.85 mHz, we note that the phenomenon causing the peaks is variable in time and in amplitude. Similar analysis of the night data shows no peaks of this kind. In addition, the shape of the power spectrum is fluctuating in time without any regular trend. Assuming that all the signal is essentially due to non-stationary atmospheric noise, we can determine its maximum amplitude in order to fix an upper limit to the confidence of the peaks in the daytime power spectra. These values are reported in Fig. 2.

Independent measurements on atmospheric noise have been performed by one of us (N. E.) at the Cassegrain focus of the 60-cm Meudon Observatory telescope with a helium cooled bolometer in the atmospheric window 8–12 μm. The experiment was performed on March 2, 1973, with a sky totally free of visible clouds. The emission measured with a 16 arc s beam had a signal to noise ratio of 140. Periodic oscillations of the signal with an amplitude of 4% that of the sky signal were observed. The power spectrum analysis of these data in the range 0 to 1 Hz with a resolution of 5 mHz shows the existence of three maxima at 7.5 mHz, 15 mHz and 26 mHz (Fig. 3). There is good coincidence of the frequency of the last two peaks with those found in the airplane spectra, obtained without any telescope and with a beam aperture of 14 arc deg.

Atmospheric transmission measurements by Biraud *et al.*<sup>3</sup>, between 300 μm and 1 mm, show in the solar spectrum an absorption peak around 8 to 9 cm<sup>-1</sup> which does not appear during night measurements on the Moon (Fig. 2 of ref. 3). Similar observations have been carried out by Nolt *et al.*<sup>4</sup> in two frequency bands. In the 7 to 40 cm<sup>-1</sup> band a feature with an apparent shape like that in our spectra is seen at about 8 cm<sup>-1</sup> (Fig. 3 of ref. 4). From the experimental parameters given in ref. 4 we conclude that this feature corresponds to

frequencies of 50 to 60 mHz. In addition, in the 5 to 20  $\text{cm}^{-1}$  band a broad absorption occurs between 8 and 10  $\text{cm}^{-1}$ . Further support comes from refs 5 and 6. Although the experimental conditions were different, anomalous absorption peaks at 7.3  $\text{cm}^{-1}$  and 9.3  $\text{cm}^{-1}$  are observed in the solar spectra which cannot be attributed to known minor atmospheric constituents. The difference between the mean absorption observed and that expected from theoretical calculations is a factor of ten. In addition, there is evidence for sky emission features at the same wavelengths at which the anomalous absorption appears.

We note that if the phenomenon which produces the features is time dependent, the peaks may appear everywhere in the submillimetre range, with a frequency defined by the experimental parameters, the length and the duration of the repetition time of each step of the interferometer moving mirror. Obviously frequency ranges in which no spectral lines are expected give better evidence of the peaks, whereas in the regions where lines occur, the feature may be superposed on the real spectrum affecting both the line intensity and shape<sup>7</sup>. Evidence of this is given by the work of Mather *et al.*<sup>7</sup> who have detected sky emission between 6 and 14  $\text{cm}^{-1}$  using a Fabry-Perot which operates in a different way from a Michelson interferometer. The expected consequence is (Fig. 2 of ref. 7) that no emission features emerged from the noise level in the frequency range 7.5 to 10  $\text{cm}^{-1}$ .

The phenomenon which produces this feature both in air-plane and ground spectra is fluctuating in amplitude and in time (periods from 20 s to 140 s), and its appearance only on day records reasonably suggests a solar origin. On the other hand, the fact that it is detected in both solar absorption and emission indicates that the source must be very large. The airplane measurements at about 1 km above the tropopause prevent any possible attribution to tropospheric fluctuations such as those studied by Kazès and Steinberg<sup>8</sup>.

The solar chromosphere, at centimetre and millimetre wavelengths has quasiperiodic pulsations correlated with the solar activity. The frequency of these fluctuations changes time with time with periods varying from 12 to 170 s (refs. 9–11).

The similarity of these periodicities with those found in our spectra suggests to us that these fluctuations may be attributed to the anomalous feature that we observe. On the other hand, the isotropy of the signal in emission suggests that the chromospheric quasiperiodic fluctuations must be coupled to the Earth through a medium able to produce this effect, such as the ionosphere.

We also note that the phenomenon we have identified has important consequences for the correct interpretation of far infrared atmospheric and astronomical data. To this end, further measurements are in progress.

E. B., on leave from the Laboratorio Plasma Spazio CNR

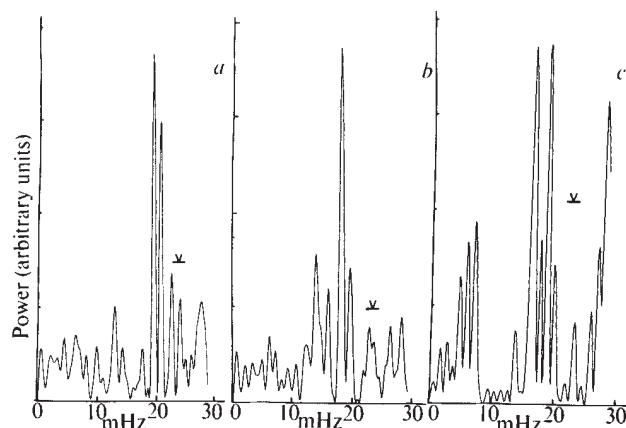


Fig. 2 Power spectra of the signal in three consecutive day measurements. Strong peaks appear respectively at a, 20.31 and 21.25 mHz; b, 19.65 mHz; c, 17.4 mHz and 19.65 mHz. The peak noise amplitude obtained from night power spectra (see text) is shown by the arrow.

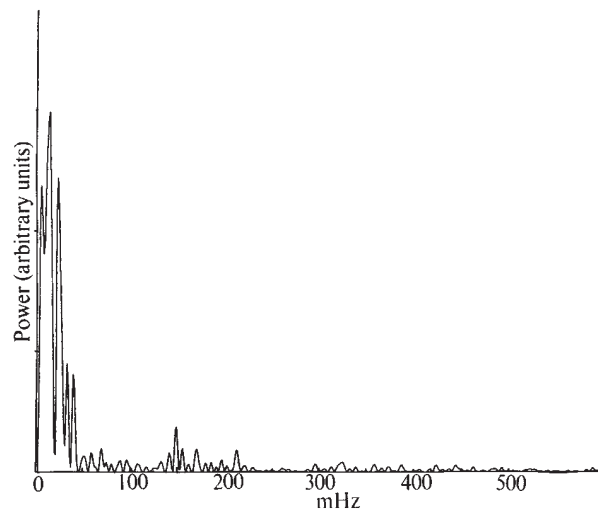


Fig. 3 Power spectrum of the sky noise data recorded on March 2, 1973, at the Cassegrain focus of the 60-cm,  $f/12.5$  Meudon Observatory telescope. Three strong maxima fall at 7.5 mHz, 15 mHz and 26 mHz.

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## Caribbean Climate during Pleistocene Ice Ages

It is a matter of controversy whether during Pleistocene ice ages non-glaciated intermediate and low latitude regions of the Earth were subjected to humid (pluvial)<sup>1,2</sup> or to arid climates<sup>3–5</sup>. We present here data on Pleistocene climates of the Caribbean basin, which may help clarify this question.

We examined a deep sea sediment core collected from the central part of the Caribbean Sea (core P6304–9; 14° 57' N, 68° 55' W, water depth 4,126 m). A detailed <sup>18</sup>O/<sup>16</sup>O palaeotemperature curve for this core indicates that it contains an undisturbed record of the late Pleistocene<sup>6</sup>. The isotopic palaeotemperature variations along the length of the core (Figs 1, 2) reflect oscillations in the temperature of seawater, which in turn are related to Pleistocene climatic fluctuations.



Other parameters measured in this core<sup>6-9</sup> all show cyclical variations which correlate closely with the palaeoclimatic oscillations.

Selected portions of the core (0 to 500 cm and 700 to 900 cm) were sampled each 10 cm; the calcium carbonate fraction of the samples was removed by treating the sediment at room temperature with a 0.5 N solution of acetic acid. The mineralogy of the carbonate-free sediment was determined by X-ray diffraction, using a General Electric XRD-5 diffractometer with Ni-filtered CuK $\alpha$  radiation. Excluding biogenous calcite, the principal minerals contained throughout the core are quartz, kaolinite, micas, plagioclase feldspars, and smectites.

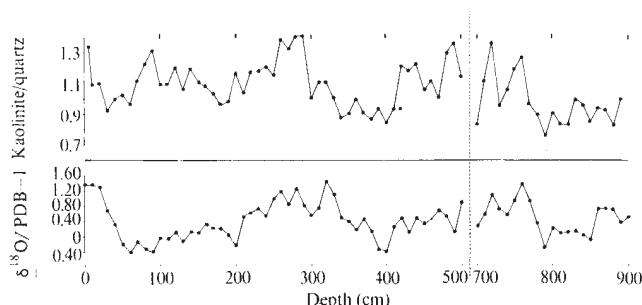


Fig. 1 Variations in the kaolinite/quartz ratio in the 0 to 500 cm and 700 to 900 cm interval of core P6304-9 compared with the  $\delta^{18}\text{O}$  palaeotemperature curve.

In Fig. 1 we show the variation with depth in two sections of the core of the ratio (in concentration) of kaolinite to quartz. In the present context we believe that this parameter can be used as an indicator of palaeoclimate. Kaolinite is a layered silicate commonly produced by chemical weathering under very humid climatic conditions. At present it is the principal clay mineral formed in humid tropical and equatorial continental areas such as in the lower basins of tropical and equatorial rivers of western Africa and South America<sup>10-12</sup>. Quartz, on the other hand, is resistant to chemical weathering and is transported into marine basins mainly from arid lands, largely through the atmosphere<sup>13,14</sup>. Thus, one would expect the kaolinite/quartz ratio to be higher in Caribbean sediments during periods of increased humidity, and lower during periods of increased aridity.

The ratios of the intensity of diffraction lines characteristic of the two minerals were measured; such ratios are proportional to the actual ratios of the concentration of the two minerals. We measured the ratios graphically, using the (001) reflexion of kaolinite at  $12.4^\circ 2\theta$  (CuK $\alpha$ ) and the (100) reflexion of quartz at  $20.9^\circ 2\theta$  (CuK $\alpha$ ), with the following instrumental parameters: 35 kV; 25 mA; divergent beam slit  $1^\circ$ ; convergent beam slit  $0.2^\circ$ ; goniometer speed  $1^\circ \text{ min}^{-1}$ . The measured ratios are reproducible to within 5%. Possible minor contributions of the (002) peak of chlorite to the (001) peak of kaolinite do not affect our conclusions.

The kaolinite/quartz ratio in the 0 to 500 and 700 to 900 cm sections of the core displays some broad fluctuations (Fig. 1) where broad minima (corresponding to periods of relatively high aridity) correlate positively with minima in Emiliani's<sup>6</sup>  $\delta^{18}\text{O}$  curve, corresponding to cold climate stages. Thus, we conclude that during Pleistocene cold stages (ice ages) conditions of relatively high aridity prevailed in the Caribbean basin, while more humid conditions prevailed during the interglacials.

Examination of samples from core P6304-9 with a high power optical microscope revealed the presence of mineral plates 3 to 4  $\mu\text{m}$  in diameter, with polygonal outline and very weak birefringence. Electron micrographs (Fig. 3), and X-ray diffraction analyses identified them as well crystallized plates of kaolinite viewed perpendicular to the (001) direction. Their

concentration was estimated at the microscope by comparison with area estimate charts each 10 cm along the length of the core (Fig. 2). There is a clear correspondence between palaeotemperature maxima and the abundance of the platy kaolinites, bearing out the concept of increased humidity in the Caribbean region during warm climatic cycles (interglacials).

The maxima of the kaolinite/quartz ratio and of the concentration of platy kaolinite are displaced upwards a few cm relative to the  $\delta^{18}\text{O}$  palaeotemperature maxima in some of the cycles (Figs 1 and 2). This could be explained if the maxima of humidity in the Caribbean basin followed the warm temperature peaks by a few thousand years; however, even if humidity maxima were contemporaneous with warm temperature peaks, the time required for chemical weathering of the circum-Caribbean regions, stripped of all chemical weathering products during the previous arid cycle, as well as for transporting the weathering products to the deep seafloor, may have resulted in a time delay between humidity maxima and corresponding maxima of the kaolinite/quartz ratio in the sediment.

The decrease with depth in the core of the average concentration of platy kaolinite (Fig. 2), and the lower average kaolinite/quartz ratio in the 700 to 900 cm section relative to the 0 to 500 cm section (Fig. 1) may be explained by the instability of kaolinite in the marine environment, and may reflect the gradual breakdown of the large plates into finer kaolinite crystals during the earliest stages of diagenesis. Further degradation of kaolinite may be slowed by burial and may take place at exceedingly low rates, possibly according to a reaction of the type: kaolinite +  $\text{K}^+$  = muscovite. Reactions of this type have been verified in the laboratory at relatively low temperature<sup>15</sup>.

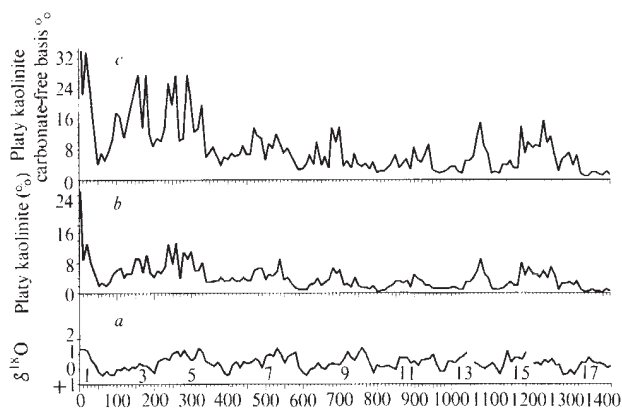


Fig. 2 Variations in platy kaolinite with depth in core P6304-9 in relation to isotopically determined palaeotemperature. a,  $\delta^{18}\text{O}$  palaeotemperature curve (from Emiliani<sup>6</sup>); b, platy kaolinite as a percentage of the whole sediment; c, platy kaolinite as a percentage of the sediment on carbonate-free basis. Recalculation on a carbonate-free basis was made using the  $\text{CaCO}_3$  data of Bostrom<sup>8</sup>.

Although our evidence cannot be applied to any one specific land area in the Caribbean region, we conclude that the provenance area of detrital sediments of the Caribbean was, in general, more arid during Pleistocene temperature minima than during temperature maxima. The inferred increased aridity of the Caribbean region during Pleistocene Ice Ages is in line with data indicating ice age aridity in the western equatorial Atlantic as well as in tropical South America<sup>10-12,16</sup>. Evidence of increased aridity during ice ages has also been found in equatorial and northern Africa<sup>5</sup> and in southern Europe and the Mediterranean<sup>3,4</sup>. Thus, it is possible that the arid climatic belt around the globe expanded during Pleistocene cold periods both towards the equator and towards the middle

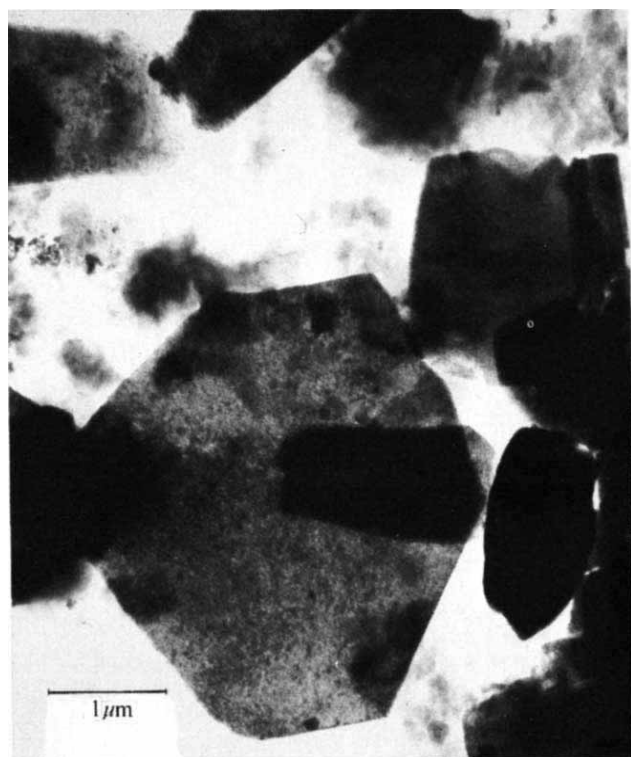


Fig. 3 Transmission electron micrographs of individual crystals of platy kaolinite from the 0 cm level of core P6304-9. The scale marker is 1  $\mu$ m. Micrographs obtained with a Philips electron microscope.

latitudes. One factor which may have contributed strongly to this phenomenon is the decreased temperature of surface water of tropical and temperate oceans which would have reduced significantly the supply of water vapour to the atmosphere in these regions.

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## Continental Drift, Magmatic Provinces and Mantle Plumes

REGIONS of magmatic activity on and in the Earth's crust have long been attributed to thermal "highs", hot spots or mantle plumes. Present doctrine<sup>1-3</sup> holds that these plumes have a fixed abode in the upper mantle, rising through the lithosphere from time to time, to "burn" holes in the crust. If the lithosphere happens to be moving as a result of active seafloor spreading, a so-called plume scar or plume trace is left in the crust, as a line of volcanoes or intrusive centres.

The thesis is useful for explaining some linear volcanic island chains, of which the best documented example remains the Hawaiian group (see ref. 3). But some recent applications of the idea to continental regions are invalid because of basic inconsistencies.

(1) The plutonic intrusions of New Hampshire (USA), the seamount chain off the New England coast, and the Azores archipelago have been linked<sup>2,4</sup> as products of a single mantle plume that first became active about 200 m.y. ago, when the North Atlantic began to open, and has continued to be intermittently active up to the present.

On this model, the 200 km long New Hampshire belt of some twenty igneous complexes, chiefly granitic (the so-called White Mountain magma series), represents the earliest phase of activity. The north-northwest trending belt is interpreted as a scar formed by movement of the North American plate over the plume, during the period from about 210 to 110 m.y. ago, corresponding to the range of radiometric ages determined for individual intrusions within the province<sup>5</sup>.

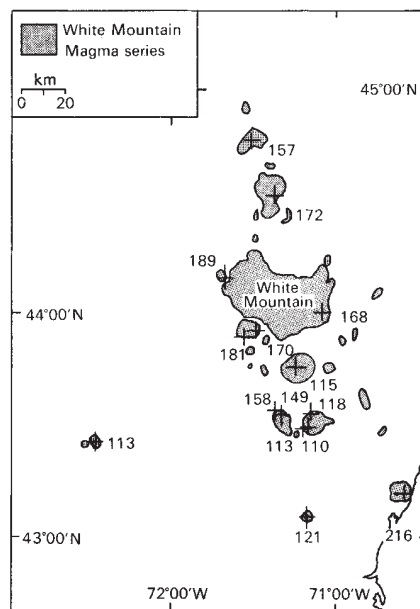


Fig. 1 Complexes of the White Mountain magma series with age data determined by Foland *et al.* +, Sample sites.

One of the axioms central to plate tectonic theory is that the direction of seafloor spreading from active ridge axes does not suddenly reverse itself nor undergo abrupt short term changes of direction through sharp angles. It follows that if the New Hampshire complexes do represent a plume scar, then they should become progressively younger towards the south-southeast. Figure 1 summarizes recently determined radiometric age data<sup>5</sup> and shows that this requirement is not met. There is no consistent pattern in the migration of centres. But the most fundamental inconsistency of this model lies in the overwhelming evidence from many sources (see ref. 6), that the North Atlantic was already about half its present width



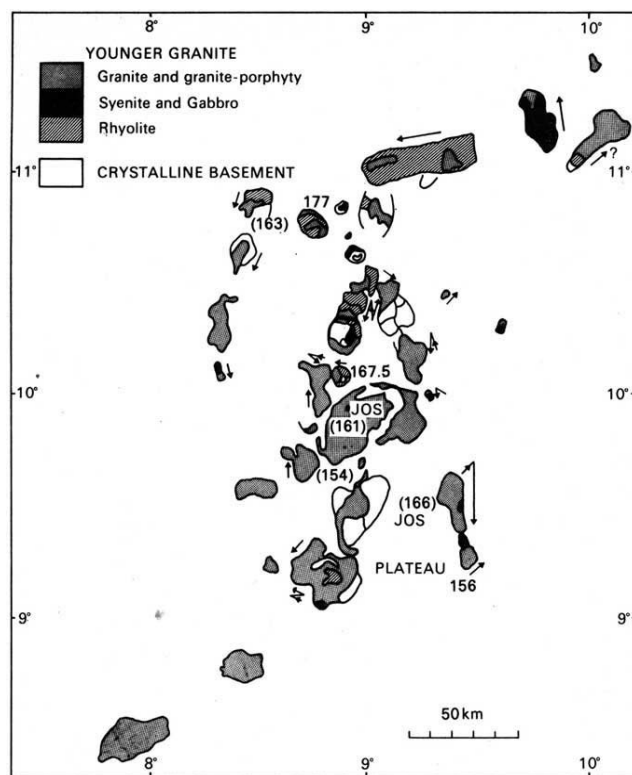


Fig. 2 Complexes of the Nigerian Younger Granite province, with age data determined by van Breemen and Bowden and by Jacobsen *et al.* (ref. 27—numbers in brackets). →, Migration of centres within complexes, taken from geological relationships established in ref. 8. There are frequent reversals of trend. There are no arrows in the central part of the Jos Plateau because the intrusions are of irregular shape and display very complex relationships. Paucity of arrows in peripheral regions is due to lack of published geological information.

by mid-Cretaceous times, the age of the youngest White Mountain intrusives—in other words the plume should by then have been beneath a point more than 1,000 km offshore.

Whatever the nature of the thermal high that gave rise to the White Mountain intrusions, it must have remained beneath New Hampshire and moved with the North American plate throughout its active life.

(2) The Jurassic Younger Granite complexes of Nigeria have been linked with Ascension Island as the products of a similar plume, that remained fixed as the African plate moved northeastward over it, during opening of the South Atlantic<sup>4,7</sup>. Figure 2 summarizes available radiometric ages<sup>7,27</sup> as well as directions of migration within individual complexes, determined from published geological maps and descriptions<sup>8</sup>.

Here again there is no consistent pattern, either in the age distribution between complexes or in the migration of centres within them, that could be taken as evidence for regarding these granites as the products of a single stationary mantle plume. Nor is there any geological evidence for linking such a plume with Ascension Island—which incidentally lies west of the Mid-Atlantic ridge, on the opposite side of the spreading axis from West Africa.

On the other hand, there is abundant geological evidence that the Nigerian Cretaceous was characterized by considerable magmatism just south of the Younger Granite province, in the Benue trough, which developed as South America and Africa finally separated around 100 m.y. ago<sup>9,10</sup>. (In this connexion note the recent convincing demonstration<sup>11</sup> that there cannot have been any Cretaceous seafloor spreading, let alone subduction, beneath the Benue trough.) In Tertiary to Recent times widespread continental alkaline basaltic and

related magmatism developed not only over the site of the Younger Granite province and in the Benue trough itself, but also in adjacent Cameroun<sup>12,13</sup>, where granitic complexes of Lower Tertiary age, petrologically similar to the Nigerian Younger granites, are also known<sup>28</sup>. This Gulf of Guinea magmatic province has been intermittently active from Jurassic times to the present, and is well known as one of Africa's major crustal swells<sup>14,15</sup>.

In both these examples, it is evident that broad thermal highs, or megaplumes, with horizontal dimensions measurable in hundreds of km, lay beneath the crust and remained active over long periods, ~100 to 200 m.y. These megaplumes moved with the lithosphere, changing shape and position to a limited extent, but in ways not obviously related to plate movements.

Local hotter spots developed within these megaplumes, generating columns of magma up to about 10 km across, very much resembling the "asthenoliths" of Belousov<sup>29</sup>. These columns formed the individual centres in the White Mountain and Jos Plateau provinces.

The basic misconception informing hypotheses such as that of Rhodes<sup>4</sup> has been first to identify each of these centres as the product of the same small stationary plume and then to use the shifting positions of those centres to chart motions of the lithosphere over the plume. In fact the distribution of activity of these small local plumes is much more likely to be controlled principally by deep lineament systems<sup>12,16,17,19</sup>, which probably also control the pattern of continental break-up<sup>18</sup> and the positions of some transform faults in oceanic crust<sup>30</sup>.

An excellent example of the distinction that must be made between the broad megaplume that underlies an extensive magmatic province and the narrow local columns (or plumes or asthenoliths) of active magma that rise from it into the crust, can be found in Iceland, where Schilling<sup>20</sup> has demonstrated that a relatively narrow and elongate megaplume underlies the central volcanic belt. Local hotter spots within the megaplume are responsible for individual volcanic outbreaks which occur unpredictably at various points along the belt, showing no discernible pattern of migration (Askja, 1961; Surtsey, 1963; Hekla, 1970; Heimaey, 1972).

The Icelandic megaplume is unquestionably one that has remained stationary with respect to moving lithosphere for at least 15 m.y., and possibly more. Further, the Hawaiian chain seems to be the product of a single rather small plume, without an associated megaplume—and the evidence is that it too has remained stationary in relation to the lithosphere. But the New Hampshire and Nigerian granite provinces as well as the Mesozoic province in southwestern Africa cited by Kröner<sup>16</sup> are equally convincing examples of megaplumes that moved long distances with the overlying lithosphere.

What are the reasons for this contrasted behaviour? Differences in the inferred shape and size and in the associated magmatism provide an obvious clue. The plume beneath Hawaii must have the form of a narrow column<sup>3</sup>, not much wider than the individual volcanoes to which it has given rise, and the predominant magma type is tholeiitic basalt. The central volcanic belt of Iceland is evidently underlain by a rather steep-sided tabular megaplume<sup>20</sup>, and the principal magmatic product is again tholeiitic basalt. The megaplumes beneath New Hampshire and Nigeria were broader and shallower, for salic magmas are by far the most abundant variety, implying a lower degree of partial melting in the upper mantle, than is required to generate basaltic magma. So the relatively hot and sharply profiled Hawaiian plume can remain stationary and pierce the moving oceanic lithosphere above it, leaving a plume trace in the form of an island chain. In the same way, the Icelandic megaplume can continue to split the lithosphere above it and contribute to seafloor spreading in the North Atlantic. By contrast, the broad, shallow and relatively cool "salic" megaplumes achieve much less penetration of the lithosphere; in consequence they tend to

remain beneath the continental swells, rather like air bubbles trapped below the gentle undulations of an ice floe. They cannot remain stationary, but must move with the lithosphere.

The trapped bubble analogy can help to illuminate the "chicken and egg" problem of whether continental swells are the cause or result of the underlying megaplumes responsible for their associated magmatism. Bailey suggests<sup>21</sup> that the swells control the magmatism, and they may result from lateral stresses set up by plate interactions<sup>9,22</sup>, a proposition for which there is some evidence<sup>31</sup>. Gentle buckling of the continental lithosphere could lead to pressure release and partial melting in the deep crust or underlying mantle, an optimum setting for generation of salic magmatism<sup>21</sup>.

The air bubbles beneath an ice floe will change shape and position in response to changes in the pattern of overlying undulations. If changes in lateral tectonic stresses with time cause changes in the pattern of continental lithosphere swells, then the broad megaplumes beneath them will change in shape and position also. For example, the immense Karroo magmatic province of South Africa, Rhodesia and Mozambique was active throughout much of Triassic and Jurassic time. The long period and diverse pattern of volcanic and intrusive activity<sup>23</sup> suggest that this region was underlain by a large and complex megaplume system of considerable longevity. It is tempting to suggest that in Cretaceous times this megaplume migrated northward in response to stresses set up during the breakup of Gondwanaland, and became responsible for the extensive salic magmatism of southern Malawi<sup>24</sup>. In the same way, the giant Kenya-Ethiopian volcanic province<sup>25,26</sup> is likely to have been underlain since Lower Tertiary times, by another megaplume which has changed its shape and position, perhaps in response to stresses accompanying collision of the African and Eurasian plates.

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## Dingle's Question

IT would be profitless to deal separately with the latest "answers"<sup>1-5</sup> to my question; their diversity tells its own tale, and the writers may see their misjudgments corrected in my book<sup>6</sup>. I have through the years put the question in various forms to meet successive obscurations, but in vain. Though wholly understandable to any ordinary layman it is at once submerged in such mathematical and metaphysical mysticism that normal readers either conclude that its simplicity must be deceptive and I deluded, or express to me their astonishment at the denseness of the "authorities". I try once more to free the stark simplicity of the matter from alien technicalities.

The "situation" is just that two exactly similar clocks, A and B, are in uniform relative motion. Then: (1) the special relativity theory says that they work at different rates, that is, one works manifestly faster than the other; (2) because they differ only in velocity the rate-difference must be directly connected with the velocity-difference, that is, the faster *working* clock must be the faster (or slower) *moving* clock; (3) the theory allows physical meaning only to *relative* velocity, so any supposed *individual* velocities of A and B can be interchanged without affecting anything observable; (4) it therefore follows from (2) that the rates of *working* are interchangeable; (5) because this would require the impossibility that A worked observably faster and slower than B, the question arises—how does the theory decide which it must do?

That is all. Every word has its ordinary meaning, there is no appeal to mathematics (all the mathematics involved is used up in deriving (1) from the postulates of the theory and is not in dispute) or to metaphysics ("time dilation", time turning into space, and so on) or to observers (every statement is objective and holds good for everyone who observes, or does not observe, anything) or to coordinate systems or frames of reference, except in the trivial sense that the phrase "supposed *individual* velocities of A and B" could, but need not, be expressed technically as "chosen coordinate system". This suffices to show that the "answers" of Jacob<sup>3</sup>, Whippman<sup>4</sup> and Stedman<sup>5</sup>, which all depend on these irrelevancies, are simply smokescreens.

The only item in the above statement which is ever challenged (almost always indirectly) is (1). The words "rate", "faster", and so on have been given esoteric meanings to make the obvious inscrutable, notwithstanding that for fifty years no one mistook their plain sense. Clearly, because words can be defined only by using other words which can themselves be challenged, this process could be endless, and a direct appeal to Einstein's writings is impossible because his words were German. I will, however, present two considerations which I think might make it difficult for anyone to challenge the layman's understanding of (1) without a blush.

Einstein made the following statement<sup>7</sup>: "If at the points P and Q" (Einstein wrote A and B, but I have used these letters otherwise) "of [the 'stationary' system] there are stationary clocks which, viewed in the stationary system, are synchronous; and if the clock at P is moved with the velocity  $v$  along the line PQ to Q, then on its arrival at Q the two clocks no longer synchronize, but the clock moved from P to Q lags behind the other which has remained at Q by" a precise amount. Here both the initial synchronization and final comparison can be made objectively by photography; the theory defines the former wholly in terms of the readings of clocks when a beam of light strikes them, which are clearly the same however the observer of the readings moves, and the latter is made when the clocks coincide. Further, I think no one (except Fremlin<sup>2</sup>) can doubt that if Einstein had attached importance to the acceleration that changes the relative velocity from 0 to  $v$ , he would not have made the literally absurd statement that the velocity was  $v$  throughout, and given a precise value for the "lag" which holds only if the acceleration is ineffective: Hence it is inescapable that,



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during the motion, one clock has, according to the theory, actually recorded a smaller interval than the other, and has therefore worked at a slower rate, which suffices to confirm (1). It also follows that (unless, indeed, nature has uniquely named each point of space) each clock has performed that feat.

Second, I consider the "twin paradox" situation, because two correspondents have dragged it in though I have repeatedly stressed that my argument is independent of it. In a recent example Paul travels at birth to a distant planet and returns 3 d old to find his earth-bound twin Peter 30 yr old. I consider only the outward journey at constant velocity up to the start of the reversing process (Paul's initial acceleration is negligible, as before, and in this case could be proved so if space allowed). My critics and I accept that because for each twin the outward and return journeys take the same time, the theory requires Paul to reach the planet 1.5 d old when Peter is 15 yr old. (It also requires these ages to be reversed, but I ignore that here, for, to establish my item (1), I keep to what is agreed). If, therefore, Paul and Peter have only apparently aged at different rates, and Paul really arrives aged 15 yr, his reversal and return must restore him from teenage to babyhood. This has not yet been explicitly claimed in defence of relativistic asymmetrical ageing (though it has implicitly), and I venture to assume that the reader will accept its impossibility as a proof that (1) is a true statement. The other items then, I think, need no defence, so the question in (5) remains unanswered.

But I must again point to the really tragic (it is not too strong a word) aspect of this matter which the infinite variety of the "answers" to my question places beyond doubt, namely, that it has become impossible for mathematical physicists to believe that this theory can be wrong. As Armstrong pertinently suggests<sup>1</sup>, if there is an answer to my very simple question, those who understand the theory would be expected to agree on what it is. Instead, one esoteric evasion after another (the new "answers" are, of course, only the tip of a many-splendoured iceberg) is served up by those who consistently ignore their predecessors and agree only in their insistence that I must be wrong. It is now easier for a camel to pass through a needle's eye than for a mathematical physicist to conceive that whatever is possible for his symbols is not of necessity possible for physical bodies. No state of mind can, in present circumstances (or, morally, in any circumstances) be more ominous than that which this controversy has shown to be practically universal among these practitioners. Once more I beg those—be they physicists, biologists, or public men of any description—in whose power it lies, to read the unmistakable signs of the times and take such steps as will ensure that in science the traditional absolute authority of reason and experience over automatic adherence to any theory, however attractive and temporarily successful, is restored before the inevitable consequences of neglecting that duty come upon us.

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## Tissue-specific Sorting-out in Two Dimensions in Relation to Contact Inhibition of Cell Movement

It is thought that the monolayering of cells which takes place on a substratum in tissue culture results from "contact inhibition of cell movement", that is inhibition of the movement of a cell in the direction of contact with another<sup>1</sup>. If it is the movement of a cell toward a contacted neighbour that is inhibited, cells which are surrounded on all sides by other cells should be immobilized<sup>2</sup>. Yet cells in heterotypic aggregates must and do move in the course of tissue-specific sorting out, even though they are surrounded in all directions by other cells<sup>3,4</sup>. Because of this we wondered whether "contact inhibition of movement" requires a solid substratum. We decided to see whether cells of different tissue origin combined in a mixed monolayer would be immobilized and prevented from sorting-out.

Four-day-old limb bud mesenchyme and 7-d-old liver from chick embryos were dissociated in 0.1% crude trypsin in calcium- and magnesium-free Hanks' solution<sup>5</sup>. The dissociated cells were mixed in minimum essential medium plus 10% horse serum and the suspension put into 35-mm tissue culture dishes so that there were just enough cells ( $8 \times 10^6$ ) to cover the dish bottom completely after spreading. The dishes were incubated at 37° C in a water-saturated atmosphere containing 5% CO<sub>2</sub>.

Examination of cultures after 48 h showed (a) that the monolayer was confluent and (b) that sorting-out had occurred. The liver cells had collected into rounded islands within a continuous phase of limb bud cells (Fig. 1). (The significance of this particular configuration will be discussed elsewhere.) Subsequent observations showed that confluency was achieved in such cultures after 10–20 h. The crucial question was whether sorting-out continued after confluency. Time-lapse cinematography and cell counting on fixed and stained monolayers showed that sorting-out began soon after the cultures had been set up, but that liver groups continued to increase in size and decrease in number after confluency was attained (at least up to 48 h). Furthermore, all cells remained in monolayer: cells did not pass over or under one another.

These findings are of interest for two reasons. First, the demonstration that cell sorting can occur in two dimensions opens up the possibility of directly observing the behaviour of sorting cells which until now has occurred obscurely in the interior of three-dimensional aggregates. Second, our results suggest that inhibition of cell locomotion is not a necessary part of the cause of monolayering. This is because the cells in our cultures were clearly able to continue sorting movements while experiencing contact inhibition (inhibition of

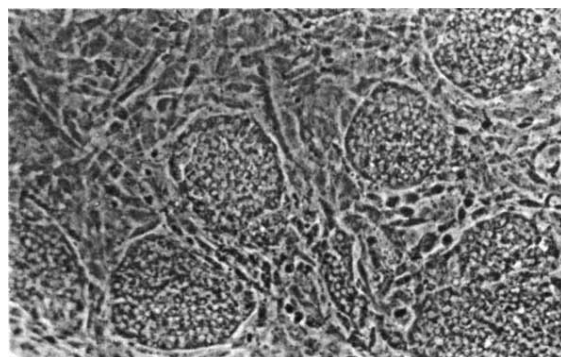


Fig. 1 Monolayer culture (48 h) of chick embryo liver and limb bud cells showing rounded liver islands within a continuous phase of limb bud ( $\times 200$ ).

overlapping) on all sides within a confluent monolayer. The films also showed that many cells exhibited both ruffling activity and movement while apparently totally and closely surrounded by other cells.

We suggest that the concept of "contact inhibition of cell movement" requires re-examination, in the sense that monolayers, developing as cells cease their forward motion upon collision with other cells, may result primarily not from an inhibition of cell movement *per se* but rather from a direct discouragement of overlapping, perhaps due to adhesive phenomena<sup>6,7</sup>.

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## Hapten-specific Helper Effects in Antibody Formation

ANTIBODY synthesis is known to result from cooperation between antigen specific bursal equivalent (B) cells and thymus-dependent (T) cells<sup>1,2</sup>. When hapten-protein conjugates are used as antigens, the T cells recognize the carrier protein determinants and the B cells produce the hapten-specific antibody<sup>3,4</sup>.

Since our original demonstration *in vivo* of hapten-specific delayed hypersensitivity to the azobenzenearsonate (ABA) group<sup>5</sup>, others have shown that this reaction also possesses many of the *in vitro* attributes of a T cell-mediated cellular response<sup>6-8</sup>, and may function as a helper in cooperating with B cells to produce antibody<sup>9</sup>.

Here, we present further evidence for the ability of ABA-specific sensitivity to perform a helper function and some additional requirements for successful collaboration between ABA-specific T cells and dinitrophenol-specific (DNP-specific) antibody producing B cells.

Polymers of L-glutamic-L-alanine-L-tyrosine (L-GAT) and D-glutamic-D-alanine-D-tyrosine (D-GAT) were coupled with DNP mustard (DNPmust) groups by the reaction of free COOH groups with a DNP-nitrogen mustard and having the structure DNP-N-(CH<sub>2</sub>)<sub>2</sub>S-(CH<sub>2</sub>)<sub>2</sub>Cl. Both D- and L-polymers were found to contain approximately  $3.3 \times 10^{-7}$  mol DNP mg<sup>-1</sup> polymer by spectrophotometric analysis.

Conjugation with ABA groups was achieved by addition of ABA-diazoniumfluoroborate salt to the DNPmust-polymer at pH 8.0. ABA content, assayed by spectrophotometric analysis, was found to be approximately  $0.8 \times 10^{-7}$  mol ABA mg<sup>-1</sup> polymer.

Skin tests with the double hapten conjugate of the L-polymer were positive in guinea-pigs immunized with ABA-N-acetyl-tyrosine in complete Freund's adjuvant (CFA), demonstrating that recognition of the ABA group was not blocked by addition

of DNPmust groups. As previously reported<sup>10</sup>, skin tests with either single or double hapten conjugates of the D-polymer were negative.

Helper function was studied in three groups of seven white male guinea-pigs (350 g). The first two groups received  $2 \times 10^{-7}$  mol ABA-Tyr in CFA in four foot pads to produce hapten-specific delayed sensitivity, and 10 µg of the DNPmust conjugate of bovine serum albumin (DNPmust-BSA) in CFA intraperitoneally to prime for antibody formation. The third group received CFA alone in the four foot pads and DNPmust-BSA in CFA intraperitoneally.

All animals were bled at intervals and sera were assayed for  $\gamma_2$  anti-DNP antibody by the passive haemolytic technique<sup>11</sup>, using tanned sheep red cells coated with DNP-human  $\gamma$ -globulin (DNP-HGG). After 6 weeks, anti-DNP titres had declined to low levels and the animals were ready for booster injections. Group 1 was given 0.5 mg ABA-DNPmust-D-GAT, half intradermally at two sites and the rest intraperitoneally. Groups 2 and 3 were given the same dose of the ABA-DNPmust-L-GAT conjugate. Skin sites were examined after 3 and 24 h.

Virtually all animals exhibited moderate Arthus reactions after 3 h, consisting of about 10 to 15 mm of oedema with some haemorrhage. This was attributable to the reaction of circulating anti-DNP antibody with DNPmust-haptens on both D- and L-polymers. By 24 h all Arthus reactions had subsided to be replaced only in Group 2 animals by delayed reactions of approximately 20 mm induration and erythema; that is, delayed reactions were produced only in the ABA-sensitized animals given ABA-DNPmust-L-polymer.

**Table 1** Anti-DNP Antibody Production in Guinea-pigs Following Booster Injection of ABA-DNPmust-L and D-GAT

| Group | Treatment                        | Boosted with      | Average anti-DNP titre |            |
|-------|----------------------------------|-------------------|------------------------|------------|
|       |                                  |                   | Pre-boost              | Post-boost |
| 1     | ABA-Tyr in CFA<br>DNP-BSA in CFA | ABA-DNPmust-D-GAT | 63                     | 80         |
| 2     | ABA-Tyr in CFA<br>DNP-BSA in CFA | ABA-DNPmust-L-GAT | 71                     | 968        |
| 3     | Saline in CFA<br>DNP-BSA in CFA  | ABA-DNPmust-L-GAT | 38                     | 48         |

One week after the booster injection, the animals were bled and the pre- and post-booster sera assayed for  $\gamma_2$  anti-DNP antibody by passive haemolysis. From the results in Table 1, it may be seen that guinea-pigs in which only B cell priming for DNP antibody production was performed (Group 3) did not show a significant boost in anti-DNP production when given the double hapten conjugate of L-GAT. The animals primed for both ABA helper cells and DNP antibody producing cells showed a marked increase in anti-DNP production following boosting with the double hapten conjugate of L-GAT (Group 2).

These results confirm that ABA sensitivity can serve as a helper function in enhancing antibody formation to the DNP hapten in suitably primed animals. In the absence of ABA sensitivity, no booster effect is obtained as the DNP moiety is recognized by the animal on two different carriers for priming and boosting and the rules of carrier specificity for an anamnestic response are in force<sup>12</sup>.

One possible reason for the different behaviour of the two polymers is that the DNP and ABA portions of the D- and L-polymers are not equally sterically accessible to the receptors on B or T cells.

Although accessibility of DNP groups to receptors on the potential antibody-producing B cells was not directly tested, it could be seen that animals exhibited anti-DNP antibody-mediated Arthus reactions of identical severity with conjugates of D- or L-polymers. Moreover, in an *in vitro* test, tanned sheep red cells coated with ABA-DNPmust-L-GAT or ABA-



DNPmust-D-GAT gave equal haemolytic titres when assayed with guinea-pig sera produced against DNP-BSA. Thus, the DNP groups on the D- and L-polymers were similarly accessible to the products of B cells, if not their receptors.

**Table 2** Inhibition by ABA-D-GAT of Stimulation *in vitro* of Lymphocytes from ABA-sensitive Guinea-pigs

| Stimulating antigen | Inhibitor               | Added at time | Stimulation index |
|---------------------|-------------------------|---------------|-------------------|
| Control             | 0                       | 0             | 1.0               |
| ABA-Tyr (100 µg)    | 0                       | 0             | 9.9               |
| ABA-Tyr (100 µg)    | ABA-poly-D-GAT (25 µg)  | -4 h          | 4.7               |
| ABA-Tyr (100 µg)    | ABA-poly-D-GAT (25 µg)  | +4 h          | 7.8               |
| None                | ABA-poly-D-GAT (100 µg) | 0             | 0.8               |

Recognition of ABA groups by receptors on T cells is more difficult to determine. We have long known that only ABA on L-polymers actively elicits all the *in vivo* or *in vitro* manifestations of cell mediated immunity. The failure of ABA conjugates of D-polymers to function cannot be attributed solely to structural features of the D-tyrosine to which the ABA group is attached, as an ABA-conjugate of D-tyrosine in an otherwise all L-amino acid polymer gives good reactions<sup>12</sup>.

To find out if ABA groups on the D-polymer can be "seen" by T cells, the possible competitive inhibition of stimulation of ABA sensitive T cells was studied. Lymph node cells from guinea-pigs sensitized with ABA-Tyr in CFA were cultured with the antigen ABA-Tyr and stimulation detected by uptake of <sup>3</sup>H-thymidine. The results of one such experiment in which varied amounts of ABA-D-GAT were added 4 h before or 4 h after the stimulating dose of ABA-Tyr are shown in Table 2. While 100 µg of ABA-Tyr alone gave a stimulation index (SI) of 9.9, addition of as little as 25 µg ABA-D-GAT gave a reduction of the SI to 4.7 when given 4 h before. Addition 4 h after stimulation had much less effect (SI = 7.8).

Thus, the ABA groups on the L-polymer can be considered to be "seen" by receptors on T lymphocytes, as judged by reactivity, whereas the ABA groups on the D-polymer can only be inferred to combine with the same receptors as measured by inhibitive activity.

The failure of the ABA-DNPmust-D-GAT polymer to produce sufficient cooperation for antibody production could be resolved by assuming that mere physical coupling of T and B cells through the binding of their receptors to suitable determinants is not a sufficient condition for antibody formation. Other studies have indicated that T cells must be activated to synthesize RNA and protein before they can initiate antibody production by B cells<sup>14</sup>. As the ABA on D-polymers cannot activate the T cell, it cannot lead to cooperative antibody production. One possible reason for this may be a requirement for some sort of preliminary processing of antigen by macrophages before T cells are activated. Some recent studies<sup>15,16</sup> have suggested that this may not occur with D-amino acid polymers.

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ON page 43 of the issue of July 6, 1973 we published a Letter by Dr Ernest B. Hook and Dr William J. Schull. In the course of sub-editing, the Letter was amended by us, but due to delays in the post the authors were unable to point out before publication that the sense of the Letter had been altered in two places. A correct version of the Letter is printed below.

The authors have also taken this opportunity to correct certain of the data in the tables which appeared in the original version.

## Why is the XX Fitter? Evidence Consistent with an Effect of X-heterosis in the Human Female from Sex Ratio Data in Offspring of First Cousin Marriages

ALTHOUGH the human female is known to be more viable, on the average, than the human male, the biological mechanisms for this difference are poorly understood. A relevant question is, to what extent does heterozygosity for X-linked loci contribute to survival of the XX female? The advantage accruing to the XX female because of this factor may be defined as X-heterosis<sup>1</sup>. Most instances of serious X-linked disorders such as haemophilia occur in males, but the total incidence of such severe diseases is relatively small and cannot account for the observed sex differences in age specific mortality rates. It is conceivable that X-heterosis is relatively insignificant in the total population, and that the bulk of the observed sex difference is attributable to physiological factors that are a consequence of sex differentiation (for example, the apparent oestrogen-sparing effect on coronary heart disease) and/or to psycho-cultural factors that may pertain to diminished exposure of woman to environmental hazards (for example, industrial pollutants).

Large sex differences in foetal death rates are not as well established as sex differences in postnatal death rates. But even if the rates of foetal loss of males and females were identical, a significant X-heterotic effect could exist in XX foetuses but be balanced by relatively beneficial physiological factors affecting primarily male foetuses.

Moreover, X-heterozygosity may not necessarily be beneficial. Evidence suggests that heterozygosity at the Xg<sup>a</sup> blood group locus in one population at least has a deleterious effect during gestation because of a dearth of heterozygous female foetuses of Xg<sup>a</sup> negative mothers<sup>2</sup>. As yet, no evidence for a large beneficial effect of X-heterozygosity in any population for

**Table 1** Livebirths, Foetal Loss and Death, by Marriage Subset\*

|                     |                  | Number of families |         | Livebirths |     | Stillbirths |   |   | Abor-tions |     | Acci-dental deaths |   | Natural deaths |       | Deaths of un-known cause |    | Twin sets |    |
|---------------------|------------------|--------------------|---------|------------|-----|-------------|---|---|------------|-----|--------------------|---|----------------|-------|--------------------------|----|-----------|----|
|                     |                  | Total              | Fertile | M          | F   | M           | F | ? | Nat.       | Th. | M                  | F | M              | F     | M                        | F  | M         | F  |
| X-Outbred<br>Type 1 | Rural koseki     | 96                 | 92      | 232½†      | 275 | 2½†         | 0 | 2 | 11         | 1   | 12                 | 2 | 49             | 52    | 3                        | 8  | 1½        | 2  |
|                     | Rural non-koseki | 18                 | 12      | 21         | 24  | 1           | 1 | 0 | 0          | 0   | 0                  | 0 | 6              | 7     | 0                        | 1  | 0         | 0  |
|                     | Urban non-koseki | 13                 | 13      | 22         | 26  | 0           | 0 | 0 | 3          | 5   | 0                  | 0 | 3              | 6     | 0                        | 0  | 0         | 0  |
|                     | Totals           | 127                | 117     | 275½†      | 325 | 3½†         | 1 | 2 | 14         | 6   | 12                 | 2 | 58             | 65    | 3                        | 9  | 1         | 2  |
| X-Outbred<br>Type 2 | Rural koseki     | 113                | 107     | 255        | 273 | 3           | 3 | 5 | 8          | 16  | 6                  | 0 | 61             | 49    | 0                        | 1  | 1         | 2§ |
|                     | Rural non-koseki | 14                 | 8       | 15         | 12  | 1           | 0 | 0 | 0          | 0   | 0                  | 0 | 4              | 1     | 0                        | 0  | 0         | 0  |
|                     | Urban non-koseki | 3                  | 2       | 2          | 1   | 0           | 0 | 0 | 0          | 0   | 1                  | 0 | 1              | 0     | 0                        | 0  | 0         | 0  |
|                     | Totals           | 130                | 117     | 272        | 286 | 4           | 3 | 5 | 8          | 16  | 7                  | 0 | 66             | 50    | 0                        | 1  | 1         | 2  |
| X-Inbred<br>Type 3  | Rural koseki     | 111                | 104     | 290        | 258 | 6           | 2 | 2 | 5          | 3   | 8                  | 2 | 63½†           | 58    | 8                        | 5  | 1         | 1  |
|                     | Rural non-koseki | 12                 | 9       | 19         | 20  | 0           | 0 | 0 | 1          | 0   | 0                  | 0 | 8              | 6     | 0                        | 1  | 0         | 0  |
|                     | Urban non-koseki | 14                 | 12      | 22         | 20  | 0           | 0 | 1 | 1          | 0   | 0                  | 0 | 3              | 0     | 1                        | 1  | 0         | 0  |
|                     | Totals           | 137                | 125     | 331        | 298 | 6           | 2 | 3 | 7          | 3   | 8                  | 2 | 74½†           | 64    | 9                        | 7  | 1         | 1  |
| X-Inbred<br>Type 4  | Rural koseki     | 143                | 129     | 340        | 330 | 6           | 3 | 2 | 17         | 6   | 7                  | 0 | 71             | 69½†  | 2                        | 0  | 1         | 1  |
|                     | Rural non-koseki | 19                 | 15      | 41         | 21  | 0           | 0 | 2 | 1          | 3   | 3                  | 0 | 10             | 10    | 0                        | 0  | 0         | 0  |
|                     | Urban non-koseki | 9                  | 7       | 24         | 12  | 0           | 1 | 0 | 0          | 2   | 1                  | 0 | 4              | 1     | 0                        | 0  | 0         | 0  |
|                     | Totals           | 171                | 151     | 405        | 363 | 6           | 4 | 4 | 18         | 11  | 11                 | 0 | 85             | 80½†  | 2                        | 0  | 1         | 1  |
| X-Inbred<br>all     | Totals           | 308                | 276     | 736        | 661 | 12          | 6 | 7 | 25         | 14  | 19                 | 2 | 159½†          | 144½† | 11                       | 7  | 2         | 2  |
| X-Outbred<br>all    | Totals           | 257                | 234     | 547½†      | 611 | 7½†         | 4 | 7 | 22         | 22  | 19                 | 2 | 124            | 115   | 3                        | 10 | 2         | 4  |

\* Pedigree and other data were collected in 1964 through a census directed at all marriages, legal or consensual, represented by at least one spouse alive and residing in Hirado at the time of the census<sup>6-7</sup>. Some 10,530 unions contracted subsequent to 1890 were ascertained, and their reproductive histories recorded. The information obtained at interview was routinely compared with the existing records of the public health office, the agricultural and fishing cooperatives, the tax office, and the kosekika, the office of custody of the koseki, the household censuses required by law in Japan<sup>8</sup>. All vital events which affect the composition of a family such as marriages, births and deaths must be reported to this office. Where there was a discrepancy between the interview and the koseki, the data from the latter were generally used. As reporting of livebirths is very complete whereas most abortions and many stillbirths go unrecorded, the observations here reported on abortions and stillbirths stem almost exclusively from interviews, whereas the livebirth data are a synthesis of interviews and koseki observations and are much more complete. Information on type of death was that obtained by interview. Most of the natural deaths were in early childhood years. About 20% of the deaths recorded were to those over 21 yr of age. Deaths of unknown cause were those which were recorded in the koseki but not recalled by those being interviewed. Almost all the deaths in this category were of individuals who had died in infancy. All twin pairs were concordant for sex, and in view of the very low rate of dizygotic twinning in Japan, each such set of twins was scored as one birth of that sex. One set of triplets resulted in two stillborn females and one liveborn male and was scored as one male livebirth and one female stillbirth (Type 2-Outbred). The following abbreviations are used: M, male; F, female; Nat., natural abortion; Th., therapeutic abortion. There was one child of unknown sex in the rural koseki group of Type 3 inbred marriage, and one natural death to a child of unspecified sex in the rural non-koseki group of Type 4 inbred marriage.

† Where one of the twins was liveborn, the other stillborn or only one died, the entry "½" is made in appropriate column (see above for rationale). ‡ This twin pair included one liveborn and one stillborn. § Both members of one twin set were stillborn.

any trait has been presented, and there are no published data available for an analysis of the net total effect of X-heterosis on differential survival in any population.

We present here data which suggest that, among the population of the Japanese Island of Hirado, X-heterosis contributed quite considerably to female survival during gestation. There is also a trend consistent with an X-heterotic effect on postnatal survival.

The theoretical basis for the approach used has been described in detail<sup>1</sup> and the discussion is limited here to first cousin offspring (see Fig. 1). Briefly, of the four types of first cousin marriages, two, types 1 and 2, may be said to be "X-outbred" as they result in XX daughters who are X-outbred and two, types 3 and 4, "X-inbred" as they result in XX daughters who are X-inbred. If X-heterosis is significant during gestation then a higher sex ratio at birth (that is of males to females) would be expected in the infants of X-inbred subsets of first cousin marriages than in those of the X-outbred subsets. And if X-heterosis is significant postnatally then X-outbred females should be relatively protected by this effect

compared with X-inbred females. The rationale for these considerations is presented in Fig. 1.

The data presented here were obtained from a large investigation of consanguineous marriages on the island of Hirado (Table 1). There were 565 unions of first cousins for which data were available; this was the largest category of consanguineous matings encountered and the only one on which a detailed analysis was undertaken. The sex ratios of livebirths, stillbirths, and postnatal deaths by first cousin subset appear in Table 1 along with details on the ascertainment and classification of the data.

The secondary sex ratios in both X-inbred subsets are higher than in both X-outbred subsets (Table 2) and the difference between the sex ratios of X-inbred subsets and X-outbred subsets is statistically significant ( $547.5/611=0.896$  against  $736/661=1.113$ ,  $\chi^2=7.24$ ,  $d.f.=1$ ,  $P<0.01$ ). If attention is restricted to the most homogenous groups within a subset, the rural, koseki checked pedigrees, the same effect is present ( $487.5/548$  against  $630/588$ ,  $\chi^2=4.65$ ,  $d.f.=1$ ,  $P<0.05$ ). The differences are thus consistent with a negative effect of X-



**Table 2** Livebirth Sex Ratio and Death Rates by Cousin Subset

|           | Livebirth sex ratio | Non-accidental death rate* |        |
|-----------|---------------------|----------------------------|--------|
|           |                     | Male                       | Female |
| X-Outbred |                     |                            |        |
| Type 1    | 0.848               | 0.221                      | 0.228  |
| Type 2    | 0.951               | 0.243                      | 0.178  |
| All       | 0.896               | 0.232                      | 0.205  |
| X-Inbred  |                     |                            |        |
| Type 3    | 1.111               | 0.252                      | 0.238  |
| Type 4    | 1.116               | 0.215                      | 0.222  |
| All       | 1.113               | 0.232                      | 0.229  |

\* The non-accidental death rate is the sum of the natural deaths and deaths of "unknown cause" (which were almost all in the infant years) divided by the number of livebirths.

**Table 3** Socioeconomic Status and Maternal Age at Birth of First Child by First Cousin Subset

|           | Socioeconomic status* |          | Maternal age at birth of first child (years) |          |
|-----------|-----------------------|----------|----------------------------------------------|----------|
|           | Mean                  | Variance | Mean                                         | Variance |
| X-Outbred |                       |          |                                              |          |
| Type 1    | 16.73                 | 36.33    | 21.60                                        | 8.38     |
| Type 2    | 18.28                 | 86.98    | 22.22                                        | 11.61    |
| X-Inbred  |                       |          |                                              |          |
| Type 3    | 16.82                 | 39.43    | 21.61                                        | 15.46    |
| Type 4    | 17.36                 | 71.43    | 21.92                                        | 13.82    |

\* Socioeconomic status is represented here by a score which is the sum of the coded values associated with parental occupation, education, and related variables. See ref. 7 for details.

**Table 4** Distribution of Families by Decade of Marriage

|                        | -1899 | 1900-1909 | 1910-1919 | 1920-1929 | 1930-1939 | 1940-1949 | 1950-1959 | 1960- |
|------------------------|-------|-----------|-----------|-----------|-----------|-----------|-----------|-------|
| X-Outbred Type 1 (127) | 2.4%  | 8.7%      | 13.4%     | 18.1%     | 10.2%     | 30.0%     | 14.2%     | 3.1%  |
| X-Outbred Type 2 (130) | 0.8%  | 3.8%      | 14.6%     | 17.0%     | 13.8%     | 28.5%     | 16.9%     | 4.6%  |
| Total X-Outbred (257)  | 1.6%  | 6.2%      | 14.0%     | 17.5%     | 12.1%     | 29.2%     | 15.6%     | 3.9%  |
| X-Inbred type 3 (136)* | 1.5%  | 8.1%      | 14.0%     | 22.8%     | 13.2%     | 26.5%     | 11.8%     | 2.2%  |
| X-Inbred Type 4 (171)  | 2.9%  | 2.9%      | 18.7%     | 17.5%     | 16.4%     | 26.3%     | 14.6%     | 0.6%  |
| Total X-inbred (307)   | 2.3%  | 5.2%      | 16.6%     | 19.9%     | 15.0%     | 26.4%     | 13.4%     | 1.3%  |

\* In one family the date of marriage is not available.

inbreeding during gestation, which leads to a net loss of females in the X-inbred subsets. There is, however, no striking difference in foetal loss between X-inbred and X-outbred subsets. Although even late foetal wastage was probably very poorly ascertained, the lack of any difference here suggests that if X-heterozygosity is responsible for the sex ratio difference at birth, females lost because of X-inbreeding may be lost early in gestation, perhaps before awareness of pregnancy, and reproductive compensation readily occurs in view of the lack of any significant effect on total fertility.

The apparent magnitude of the effect is relatively large, and it may be specific to the population under investigation. In addition, it is present in spite of any influence polymorphism at the *Xg<sup>a</sup>* locus might have had on sex ratio in those studied; but we have no data on the frequency of such alleles at this locus on the island. Furthermore, the observed sex-ratio in

the X-outbred group is not necessarily what would be predicted in a completely outbred control population, as sex differential responses to autosomal inbreeding in both X-inbred and X-outbred subsets may also have had an effect on the sex ratios in those studied here.

Another trend of interest is the postnatal death rate. If X-heterosis is significant for postnatal survival then X-inbred females should have greater mortality and morbidity than X-outbred females, but there should be no effect in the males. What data are available are consistent with this hypothesis. Although deaths have probably been poorly ascertained because of migration and other factors, most of those recorded represent deaths in infancy and early childhood. The mortality rates for non-accidental deaths (as defined in Table 2) in males in the two groups are identical, 0.232. But the recorded death rate in X-inbred females, 0.229, is about 12% greater than that in X-outbred females, 0.205. With the number of individuals involved, however, the trend is not yet significant at the 5% level.

Most studies to date of offspring of consanguineous unions have compared them with outbred individuals, and conclusions may always be vitiated by possible systematic differences between the related and unrelated families. Our study, by making comparisons within the inbred group of first cousin offspring, avoids many of these pitfalls. It is still possible, however, that some systematic differences between the subsets might have contributed to the differences in sex ratios. Comparisons of X-inbred groups and X-outbred groups with regard to a number of possibly relevant factors are presented in Tables 3-6. There appear to be no major differences with regard to decade of marriage, geographical distribution on the island,

**Table 5** Distribution of Families by Religion

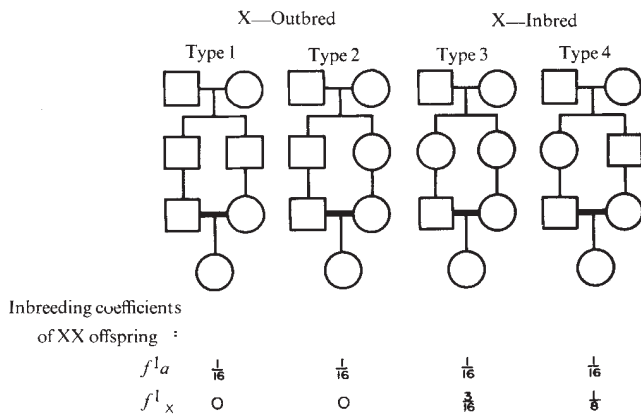
|                       | Buddhist | Catholic | Kakure* | Other |
|-----------------------|----------|----------|---------|-------|
| X-Outbred -1 (127)    | 81.1%    | 0.8%     | 17.3%   | 0.8%  |
| X-Outbred -2 (130)    | 80.0%    | 0%       | 16.2%   | 3.8%  |
| X-Outbred total (257) | 80.5%    | 0.4%     | 16.7%   | 2.3%  |
| X-Inbred -3 (135)     | 79.3%    | 1.5%     | 17.0%   | 2.2%  |
| X-Inbred -4 (170)     | 83.5%    | 0.6%     | 12.4%   | 3.5%  |
| X-Inbred total (305)† | 81.6%    | 1.0%     | 14.3%   | 3.0%  |

\* A syncretic cult incorporating elements of Buddhism and Catholicism.

† 2 intermarriages are excluded, and data are not available on one family.

**Table 6** Distribution of Families by Geographic Area

|                        | Hirado -1 | Hirado -2 | Nakano | Himosashi | Shishi | Nakatsura | Tsuyoshi | Shijiki |
|------------------------|-----------|-----------|--------|-----------|--------|-----------|----------|---------|
| X-Outbred Type 1 (127) | 9.4%      | 25.2%     | 13.4%  | 10.2%     | 20.5%  | 7.1%      | 6.3%     | 7.9%    |
| X-Outbred Type 2 (130) | 9.2%      | 17.7%     | 9.2%   | 22.3%     | 18.5%  | 10.0%     | 7.7%     | 5.4%    |
| Total X-outbred (257)  | 9.3%      | 21.4%     | 11.3%  | 16.3%     | 19.5%  | 8.6%      | 7.0%     | 6.6%    |
| X-Inbred Type 3 (137)  | 10.9%     | 22.6%     | 15.3%  | 12.4%     | 16.1%  | 3.6%      | 8.0%     | 10.9%   |
| X-Inbred Type 4 (171)  | 9.9%      | 17.5%     | 7.6%   | 18.1%     | 15.1%  | 7.0%      | 12.3%    | 12.3%   |
| Total X-inbred (308)   | 10.4%     | 19.8%     | 11.0%  | 15.6%     | 15.6%  | 5.5%      | 10.4%    | 11.7%   |



**Fig. 1** Possible pedigrees of XX offspring of first cousin marriages. Note that XXs born to first cousins have identical autosomal inbreeding coefficients but may have different X chromosome inbreeding coefficients depending on the parental relationship. If heterozygosity for X-linked loci has an effect on a particular attribute, say, survival, this effect should be less for X-inbred females (that is, those for whom  $f^1_x > 0$ ) who will be more homozygous for X-linked loci, on the average, than females for whom  $f^1_x = 0$ , that is, X-outbred females. If X-heterozygosity is beneficial, this advantage attributable to outbreeding should by definition be more significant for X-outbred than X-inbred females but would have no effect on XY males. Therefore, if X-heterozygosity contributes positively to survival to age  $n$ , the sex ratios (M/F) of X-outbred groups (that is, those in which X-outbred females occur, types 1 and 2 above) should be less than those of the X-inbred groups (those in which X-inbred females occur, types 3 and 4) and conversely if there is a negative effect. The secondary sex ratios, that is those noted at birth, should reflect the net effect of X-heterozygosity during gestation. The occurrence of XO females and males with more than one X-chromosome will confound the theoretical expectation, but the instances of such individuals in all populations studied to date appear to be less than 0.2% and are thus likely to be of trivial magnitude. See refs 1, 3 and 4 for further discussion.

maternal age at birth of the first child, socioeconomic status, or religion. This does not exclude the possibility of some other as yet unknown differences between these subsets that might be associated with altered sex ratio. At present, however, the only known systematic difference between these groups is the X-inbreeding coefficients of their female members, suggesting that this is the causal factor for the sex ratio data.

These observations provide, to our knowledge, the first evidence consistent with a significant deleterious effect of X-inbreeding in any mammalian population.

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## Test of Selection and Neutrality in Natural Populations

MORE than a century after the publication of Darwin's *Origin of Species*, the question of the relative contribution of selected and nonselected ("neutral") genetic variation to evolution is still unresolved<sup>1,2</sup>.

A test of the theory of selective neutrality of polymorphisms has recently been suggested by Lewontin and Krakauer<sup>3</sup>, on the basis of an idea first introduced by Cavalli-Sforza<sup>4</sup> in analysing human evolution. The method is based on the contrasting effects of natural selection and the breeding structure on allele frequencies. Whereas natural selection operates differentially on each allele, inbreeding affects all alleles similarly. The inbreeding effect can be calculated in populations irrespective of their degree of isolation or their actual breeding structure, thereby avoiding manipulations with the parameters of population size,  $N$ , mutation rate,  $u$ , migration rate,  $m$ , and selection intensity,  $s$ , which are largely unknown in natural populations<sup>3</sup>.

Spatio-temporal variation in steady state gene frequencies, in the absence of selection, reflects the breeding system alone and can estimate a parameter  $\hat{F}_e$  the "effective inbreeding coefficient" across populations, or the "standardized variance of gene frequency"<sup>4</sup>.  $F_e$  is an equilibrium value reached after many generations and involves selection and random drift. The estimate is  $\hat{F}_e = S^2_p / \bar{p}(1 - \bar{p})$  where  $\hat{F}_e$  is the estimate of effective inbreeding,  $S^2_p$  is the variance in the frequency of one one of two alternative alleles across populations<sup>3</sup>, and  $\bar{p}$  is the mean frequency of the allele across populations. Selectively neutral alleles will all have similar estimated  $\hat{F}_e$  values in spite of their individual variation in  $S^2_p$  and  $\bar{p}$  because effective inbreeding will be identical for all genes across populations. The average  $\hat{F}_e$  will thus estimate the true  $F_e$  without significant heterogeneity among alleles. If, however, selection is operating on some or all of the loci, their  $\hat{F}_e$  values will be significantly heterogeneous and will not be estimates of the same  $F_e$ . The test of homogeneity of  $\hat{F}_e$  estimates from different loci in steady state is thus a test for selection. The statistical properties of  $\hat{F}_e$  and a sampling theory for testing heterogeneity in  $\hat{F}_e$  are given in Lewontin and Krakauer<sup>3</sup>.

Here I describe a test by the  $\hat{F}_e$  method for selection and/or non-selective factors operating in natural populations of the fossorial rodent *Thomomys talpoides*, which belongs to the pocket gopher family *Geomyidae*. The basic data on gene frequencies are taken from ref. 5.

Alloenzymatic variation in proteins encoded by 31 loci was analysed electrophoretically in 276 specimens representing eleven populations and six chromosome forms of *Thomomys talpoides* complex of pocket gophers in the southern Rockies of the USA<sup>5</sup>. The eleven populations, their localities, abbreviations and respective diploid numbers are as follows. (1) Snowy Range Pass (SR), S.E. Wyoming,  $2n=48$ ; (2) Willow Creek Pass (WC), N. Colorado,  $2n=46$ ; (3) Dead Horse Creek (DH), head of White River, N. Colorado,  $2n=44$ ; (4) Three Forks Campsite (TF), N.W. Colorado,  $2n=40$ ; (5) Kaibab Plateau (KP), N. Arizona,  $2n=40$ ; (6) La Sal Mountains (LS), E. Utah,  $2n=60$ ; (7) Molas Lake (ML), S.W. Colorado,  $2n=60$ ; (8) Canjilon (CN), N. New Mexico,  $2n=48$ ; (9) Redondo, Jemez Mountains (RD, JM), N. New Mexico,  $2n=48$ ; (10) Las Conchas, Jemez Mountains (LC, JM),



N. New Mexico,  $2n=48$ ; (11) Mount Taylor (MT), N. New Mexico,  $2n=48$ . Karyotypes 48 Wyoming ( $=48 W$ ), 46 and 44 are represented each by one population only. Karyotypes  $2n=40$  and  $2n=60$  are represented by two isolated populations each, while  $2n=48$  New Mexico ( $=48 NM$ ), which differs in morphology from  $2n=48 W$ , is represented by four populations, CN, RD-LC (both in the Jemez Mountains, separated altitudinally) and MT. CN is isolated from (RD+LC) and they are both isolated from MT. Detailed accounts of all localities are given in ref. 5. Thaler (ref. 6 and unpublished work) has shown in extensive karyotypic analysis of the *Thomomys talpoides* complex that where numerous differences in karyotype exist, either in number or morphology of chromosomes, the karyotypes coexist parapatrically with little or no inter-

breeding. From this he assumes, on distributional evidence, that the six karyotypes involved in this study represent at least five distinct species whose diploid numbers are  $2n=48 W$ , 44-46 (which may or may not be conspecific, no evidence is yet available), 40, 60 and 48 NM.

The distribution of commonest allele at twenty-four polymorphic loci in the eleven populations, together with their respective  $\hat{F}_e$  values are given in Table 1. The loci in Table 1 are arranged in three groups\*. Group I, glucose metabolizing enzymes; Group II, all other enzymes; and Group III, non-enzymatic proteins. Alleles are designated alphabetically in order of decreasing mobility of their alloenzymes. Of the thirty-one loci examined eight are monomorphic in all eleven populations for the same allele; seven are fixed for the same

**Table 1** Frequency of Commonest Allele at Twenty-four Polymorphic Loci in Eleven *Thomomys talpoides* Populations Together with their  $\hat{F}_e$  Values

|                                       |               | Populations      |                  |                  |                  |                  |                  |                  |                  |                  |                         | Effective<br>inbreeding<br>coefficient( $F_e$ )† |                  |
|---------------------------------------|---------------|------------------|------------------|------------------|------------------|------------------|------------------|------------------|------------------|------------------|-------------------------|--------------------------------------------------|------------------|
| Locus<br>and<br>allele*               | $2n =$<br>N = | SR<br>48<br>(30) | WC<br>46<br>(30) | DH<br>44<br>(26) | TF<br>40<br>(24) | KP<br>40<br>(30) | LS<br>60<br>(24) | ML<br>60<br>(25) | CN<br>48<br>(30) | RD<br>48<br>(14) | JM†<br>LC<br>48<br>(15) |                                                  | MT<br>48<br>(28) |
| Group I: glucose metabolizing enzymes |               |                  |                  |                  |                  |                  |                  |                  |                  |                  |                         |                                                  |                  |
| <i>Mdh-1</i>                          | a             | 1.0              | 1.0              | 1.0              | 0.98             | 1.0              | 0.94             | 1.0              | 1.0              | 1.0              | 1.0                     | 1.0                                              | 0.059            |
| <i>aGpd</i>                           | b             | 1.0              | 0.95             | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 0.95             | 1.0              | 1.0                     | 1.0                                              | 0.041            |
| <i>Ldh-1</i>                          | b             | 1.0              | 0.90             | 1.0              | 1.0              | 1.0              | 1.0              | 0.98             | 0.00             | 0.00             | 0.00                    | 0.00                                             | 0.957            |
| <i>Ldh-2</i>                          | b             | 1.0              | 0.87             | 0.90             | 1.0              | 1.0              | 0.92             | 1.0              | 1.0              | 1.0              | 1.0                     | 0.70                                             | 0.155            |
| <i>Idh-1</i>                          | b             | 1.0              | 1.0              | 1.0              | 1.0              | 0.65             | 1.0              | 1.0              | 0.98             | 1.0              | 1.0                     | 0.00                                             | 0.794            |
| <i>6Pgd</i>                           | b             | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 0.35             | 0.00             | 1.0              | 0.93             | 0.73                    | 0.91                                             | 0.658            |
| <i>Got-1</i>                          | c             | 1.0              | 1.0              | 1.0              | 1.0              | 0.90             | 1.0              | 1.0              | 0.98             | 1.0              | 1.0                     | 1.0                                              | 0.077            |
| <i>Got-2</i>                          | a             | 1.0              | 1.0              | 0.98             | 1.0              | 1.0              | 1.0              | 1.0              | 0.98             | 1.0              | 1.0                     | 1.0                                              | 0.017            |
| <i>Pgm</i>                            | c             | 0.88             | 0.92             | 1.0              | 1.0              | 0.00             | 1.0              | 1.0              | 1.0              | 0.89             | 0.89                    | 1.0                                              | 0.696            |
| <i>Pgi</i>                            | a             | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 0.65             | 1.0              | 1.0              | 1.0              | 1.0                     | 1.0                                              | 0.329            |
| <i>Tpi</i>                            | b             | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 0.88             | 0.71             | 0.60                    | 1.0                                              | 0.265            |
| Group II: other enzymes               |               |                  |                  |                  |                  |                  |                  |                  |                  |                  |                         |                                                  |                  |
| <i>Sdh</i>                            | a             | 1.0              | 1.0              | 1.0              | 0.92             | 1.0              | 0.98             | 1.0              | 0.98             | 1.0              | 1.0                     | 1.0                                              | 0.050            |
| <i>Pept.</i>                          | b             | 0.52             | 1.0              | 0.98             | 1.0              | 0.00             | 1.0              | 0.88             | 1.0              | 1.0              | 1.0                     | 1.0                                              | 0.729            |
| <i>Acph</i>                           | b             | 0.68             | 1.0              | 1.0              | 0.98             | 1.0              | 0.19             | 1.0              | 0.98             | 1.0              | 1.0                     | 1.0                                              | 0.607            |
| <i>Es.-2</i>                          | b             | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 0.55             | 0.36             | 0.33                    | 0.14                                             | 0.589            |
| <i>Es.-3</i>                          | b             | 1.0              | 0.85             | 0.73             | 0.63             | 0.22             | 0.58             | 0.98             | 0.98             | 0.61             | 0.50                    | 0.68                                             | 0.249            |
| <i>Es.-4</i>                          | c             | 0.45             | 0.92             | 0.96             | 0.77             | 0.00             | 0.88             | 0.98             | 0.87             | 1.0              | 0.93                    | 0.91                                             | 0.495            |
| <i>Ak-1</i>                           | a             | 1.0              | 1.0              | 1.0              | 1.0              | 0.00             | 1.0              | 1.0              | 1.0              | 1.0              | 1.0                     | 1.0                                              | 0.091            |
| <i>Ak-2</i>                           | c             | 1.0              | 0.72             | 0.98             | 0.00             | 1.0              | 0.00             | 0.00             | 1.0              | 0.96             | 0.97                    | 1.0                                              | 0.081            |
| Group III: nonenzymatic proteins      |               |                  |                  |                  |                  |                  |                  |                  |                  |                  |                         |                                                  |                  |
| <i>Hb</i>                             | b             | 1.0              | 1.0              | 0.90             | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 1.0                     | 1.0                                              | 0.092            |
| <i>Preal-1</i>                        | b             | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 1.0              | 0.65             | 0.93             | 1.0                     | 0.55                                             | 0.326            |
| <i>Preal-2</i>                        | a             | 1.0              | 0.98             | 0.92             | 1.0              | 1.0              | 0.83             | 1.0              | 1.0              | 0.96             | 1.0                     | 1.0                                              | 0.094            |
| <i>Alb.</i>                           | a             | 0.00             | 0.62             | 0.73             | 1.0              | 1.0              | 1.0              | 1.0              | 0.00             | 0.00             | 0.00                    | 0.52                                             | 0.750            |
| <i>Trf.</i>                           | b             | 1.0              | 1.0              | 0.98             | 1.0              | 1.0              | 0.94             | 1.0              | 0.98             | 1.0              | 0.96                    | 1.0                                              | 0.361            |

Proteins are arranged in three groups: group I, glucose metabolizing enzymes; group II, other enzymes; group III, nonenzymatic proteins.

\* Abbreviations: *Mdh-1* malate dehydrogenase 1; *aGpd*: alpha glycerophosphate dehydrogenase; *Ldh-1*: Lactate dehydrogenase-1; *Ldh-2*: Lactate dehydrogenase-2; *Idh-1*: Isocitrate dehydrogenase-1; *6Pgd*: 6 phosphogluconate dehydrogenase; *Got-1* and 2: Glutamic-oxaloacetic transaminase 1 and 2; *Pgm*: Phosphoglucomutase; *Pgi*: Phosphoglucose isomerase; *Tpi*: Trisphosphate isomerase; *Sdh*: Sorbitol dehydrogenase; *Pept.*: Peptidase; *Acph*: Acid phosphatase; *Es.-2*: Esterase-2; *Es.-3*: Esterase-3; *Es.-4*: Esterase-4; *Ak-1*: Adenylate kinase-1; *Ak-2*: Adenylate kinase-2; *Hb*: Haemoglobin; *Preal-1*: Prealbumin-1; *Preal-2*: Prealbumin-2; *Alb.*: Albumin; *Trf.*: Transferrin.

† JM involves two subpopulations.

‡ Corrected for sampling error by subtracting  $1/n=0.04$  from the observed  $\hat{F}_e$ ; mean  $n=25$ .

**Table 2** Summary of Four Tests for the Heterogeneity of  $\hat{F}_e$  by Comparison of Observed and Theoretical Variances for all Major Alleles and Three Groups of Proteins in *Thomomys talpoides*

| Group                                  | Alleles (N) | Effective inbreeding coefficient |                                                 | $S^2_F/\sigma^2$ | Critical F value with appropriate degrees of freedom | P                            |
|----------------------------------------|-------------|----------------------------------|-------------------------------------------------|------------------|------------------------------------------------------|------------------------------|
|                                        |             | Observed* mean ( $\bar{F}$ )     | Theoretical variance ( $S^2_F$ ) ( $\sigma^2$ ) |                  |                                                      |                              |
| All major alleles                      | 24          | 0.317                            | 0.079                                           | 0.020            | 3.94                                                 | $F_{0.001}(23, \infty)=2.13$ |
| Group I (Glucose metabolizing enzymes) | 11          | 0.328                            | 0.119                                           | 0.021            | 5.52                                                 | $F_{0.001}(10, \infty)=2.84$ |
| Group II (Other enzymes)               | 8           | 0.321                            | 0.066                                           | 0.021            | 3.19                                                 | $F_{0.001}(7, \infty)=3.47$  |
| Group III (Nonenzymatic proteins)      | 5           | 0.285                            | 0.072                                           | 0.016            | 4.47                                                 | $F_{0.001}(4, \infty)=4.62$  |

\* Corrected for sample size.

allele (*Mdh-2*, *Idh-2*, *Adh*, *Ipo*, *Es-5*, *Prot-1* and *Hp* (these do not appear on Table 1); while *Ak-1* is fixed for the same allele in nine populations but for an alternative one in the KP population (Table 1). Most loci tend to monomorphism in reproductively as well as geographically isolated populations. This results in relatively low heterozygosity ( $\bar{H}$ ), 0.047 (range, 0.008–0.085), which is even lower if the highly polymorphic esterases (*Es-3* and *Es-4*) are excluded ( $\bar{H}=0.037$ , range, 0.005–0.067). A remarkable feature is the weak regional polymorphism in most populations. Most loci, either monomorphic or weakly polymorphic have the same, either fixed or predominant allele. Alternative fixation between or within karyotypes, is relatively uncommon. Finally, high genic similarity characterizes karyotypes<sup>5</sup>.

The  $\hat{F}_e$  values are distinctly heterogeneous, running from very low to very high values. This apparent heterogeneity of  $\hat{F}_e$  was tested by comparing the observed (corrected for sampling error) and theoretical variances first for all twenty-four alleles, then for each of the above mentioned protein groups separately; the results are summarized in Table 2. The test performed was the conservative one using  $k=2.0$  for deriving the theoretical variance, which is appropriate for binomial gene distribution<sup>3</sup>. The number of populations in the analysis is  $n=11$ , from this the theoretical variance,  $\sigma^2=k\bar{F}/n-1$  was calculated by  $\sigma^2=2.0\bar{F}/10$ , where  $\bar{F}$  or mean  $\hat{F}_e$ , is given for each group separately in Table 2. The ratios between the observed and theoretical variances of inbreeding coefficients is also given in Table 2 together with the critical values of the  $F$  distribution, which is distributed as  $\chi^2/d.f.$ <sup>3</sup>.

The ratio of observed and theoretical variance (Table 2) indicates excess heterogeneity of  $\hat{F}_e$  values for all alleles tested as well as for each of the three protein groups. The ratio decreases from group I to III and then group II, being 5.52, 4.47 and 3.19, respectively. This heterogeneity can be taken as evidence for selection. Furthermore, the differences in the values of  $\hat{F}_e$  may suggest the type of selection operating<sup>3</sup>. Uniform selection across the range will result in a low variance and so in low  $\hat{F}_e$  values. This is certainly true for alleles which are either prevalent or close to fixation across populations (*Mdh-1*, *aGpd*, *Got-1*, *Got-2*, *Sdh*, *Ak-1* and the monomorphic loci which are not analysed here as they do not vary across the range). On the other hand, diversifying selection will result in high variance among populations, suggesting differential selection in different environments. This seems to be the case in the strongly polymorphic loci (esterases) a fact which is displayed by the geographical, clinal variation of *Es-2* and *Es-3*. High  $\hat{F}_e$  values also occur when alternative fixation of alleles takes place either among karyotypes (*Ldh-1*, *Alb*) or among populations within a karyotype (*Idh-1*, *6Pgd*, *Pgm*, *Pept*).

The ranking of heterogeneities of  $\hat{F}_e$ , is group I > group III > group II. This finding is inversely correlated with mean heterozygosity per individual ( $\bar{H}$ ) which is 0.037, 0.042 and 0.061 respectively<sup>5</sup>. The differential  $\bar{H}$  found among the three groups, particularly between groups I and II, is consistent with similar findings in *Drosophila*<sup>7-9</sup>, though apparently inconsistent with data for vertebrates<sup>10</sup>. Gillespie and Kojima<sup>7</sup> predicted that glucose metabolizing enzymes would be less variable than peripheral enzymes because their physiological function is more specific than that of group II enzymes. Further support for the hypothesis that the three protein groups may partly be under differential selective pressures is suggested by a multiple regression analysis of the variance in their  $\bar{H}$  (ref. 5).

The six karyotypes studied are distinctly similar genetically,  $I^{11}=0.925$  (range, 0.859–0.996) (ref. 5), yet migration can be completely ruled out to explain similarities. First, field evidence suggests complete reproductive isolation between karyotypes such as  $2n=60$  and  $2n=48$  NM (populations ML and CN), or between  $2n=60$  and  $2n=48$  NM (populations ML and CN), or between  $2n=44$  and  $2n=40$  (DH and TF), and nearly so between  $2n=46$  and  $2n=48$  W (ref. 6 and C. S. Thaeler, unpublished). Second, the isolated populations

at the southernmost fragmented portion of the *T. talpoides* range (KP, LS, JM, MT) are completely isolated geographically on mountain tops, with vast intervening lowlands occupied by *Thomomys bottae* which competitively excludes, and is reproductively isolated from *T. talpoides*<sup>12</sup>. The genic similarity among the six karyotypes, despite their reproductive and/or geographical isolation strongly supports selection as an important cause of genetic differentiation in *Thomomys talpoides* populations.

The general pattern of low heterozygosity that characterizes pocket gophers as well as other fossorial rodents<sup>13</sup> is best explained as an adaptive strategy for homozygosity in the relatively uniform, but narrow-niche underground environment. Strong selection for homozygosity may particularly be operating on alleles which did not diverge across the range in spite of reproductive and ecogeographical isolations in the late and post-Pleistocene period.

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## Number of Gene Loci required for Accurate Estimation of Ancestral Population Proportions in Individual Human Hybrids

GENETIC studies giving estimates of the Caucasian genetic contribution to American blacks have recently been reviewed and extended<sup>1</sup>. These group admixture estimates are mean population values; but estimates of admixture proportions for individuals of hybrid origin, using genetic markers, have not been published. Recent interest in this possibility, however, has led to an explicit proposal for making such individual estimates<sup>2,3</sup>. This communication is not concerned with the reasons for making such estimates nor with the uses to which they may be put. Its purpose is to demonstrate, by a simple argument, that the number of genetic marker loci required for reasonably accurate individual admixture estimates seems to be so large that such estimates cannot be made now or in the near future.

To make the argument as simple as possible and avoid intractable complexities (which we discuss later), I postulate an ideal estimation situation. Because real estimation will



not be as favourable, the estimate from the ideal model of the number of loci will necessarily represent the minimum. We make the following five assumptions. (i) The hybrid population  $H$  is derived from two genetically different ancestral populations,  $P_1$  and  $P_2$ . (ii) There is negligible selection, mutation, and genetic drift at the marker loci used in estimating  $\mu_i$  (the true proportion of genes in the  $i$ th individual in  $H$  which derive from  $P_1$ ;  $m_i$  is an estimate of  $\mu_i$ ). (iii) The marker loci used for estimating  $\mu_i$  are representative of all loci in indicating population origin. (iv) Each allele at each marker locus in a person in  $H$  is individually classifiable, without error, as coming from either  $P_1$  or  $P_2$  (that is, there is no dominance, epistasis, or misclassification and  $P_1$  and  $P_2$  are homozygous, for different alleles, at each marker locus). (v) The marker loci used assort independently at meiosis (there is no close, moderate, or loose linkage between any pair of these loci so that each locus gives an equal, independent, and maximal amount of information about ancestral population origin). Because we are not concerned here with the processes which have produced a given  $\mu_i$ , we need say nothing about the mating patterns in  $H$ .

Given that these five assumptions hold, a person with  $n$  ideal marker loci has  $2n$  ideal genes, each classifiable as coming from  $P_1$  or  $P_2$ .  $m_i$ , estimating  $\mu_i$ , is therefore the proportion of these  $2n$  genes coming from  $P_1$ . The variance of  $m_i$ ,  $s_i^2$ , is  $\mu_i(1-\mu_i)/2n$ . We can specify the accuracy of  $m_i$  by its standard error,  $s_i$ . Therefore,

$$n = \mu_i(1 - \mu_i)/2s_i^2 \quad (1)$$

For a specified  $s_i$ , which we may call  $s$ ,  $n$  varies with  $\mu_i(1 - \mu_i)$ .  $\mu_i$ , in persons in  $H$ , can vary between 0 and 1 and  $\mu_i(1 - \mu_i)$  has its maximum when  $\mu_i = 0.5$ . The expected value of  $\mu_i$  is about 0.22 for non-Southern American blacks and about 0.1, or slightly more, for most Southern American blacks<sup>1</sup>. To ensure that the estimate of  $n$  for a majority of American blacks is usually a minimum estimate,  $n$  is estimated with  $\mu_i = 0.1$ . For such a minimum estimate,  $n_{min}$  therefore

$$n_{min} = 0.045/s^2 \quad (2)$$

The value of  $s$  for "accurate" estimation depends on the definition of "accurate" and is necessarily arbitrary. In my opinion  $s$  should not exceed 0.05, corresponding to a 95% confidence interval of about 0.20.

Using  $s = 0.05$ , equation 2 estimates  $n_{min}$  to be eighteen ideal loci (thirty-six ideal genes). For this number, a  $m_i$  of 0.10 has a 95% confidence interval of about 0.025–0.25 (ref. 4). This minimum estimate gives only minimal accuracy. If we wish to reduce the 95% confidence interval to a width of about 0.10, corresponding to  $s = 0.025$ , we would require (for  $\mu_i = 0.1$ ) seventy-two ideal loci as a minimum. To interpret these estimates we note that only one or two known marker loci of man approach the ideal in population discrimination. Even the best population marker, the  $Gm$  locus, falls short of having high frequency, distinctive alleles in each major racial group<sup>5</sup>. When comparison is only between African populations ancestral to American blacks (West Africa and the western coast, south to Southwest Africa<sup>1</sup>), and Caucasians, and detailed testing is done,  $Gm$  is almost ideal because neither population shares common alleles<sup>5</sup>. The Duffy locus is almost as good for this comparison when both anti-Fy<sup>a</sup> and anti-Fy<sup>b</sup> are used<sup>1</sup>. Most of the approximately fifty marker (polymorphic) loci known in man make much less sharp distinctions for this comparison or for other racial comparisons.

The relative usefulness of a particular gene  $g$ , present in a dihybrid individual  $I$ , for providing information on the ancestral population origins of that gene is a simple function of the frequencies of  $g$  in the two ancestral populations. The following illustration shows how greatly two genes, each seemingly discriminative, may differ from each other in their usefulness for discriminating between such ancestral populations. The frequencies of the Fy<sup>a</sup> gene of the Duffy blood group system in Caucasians and Africans (of the former slaving areas) are

about 0.43 and about 0.01, respectively, while the corresponding frequencies of the  $A$  gene of the ABO blood group system are about 0.40 and 0.15<sup>1,6</sup>. The population difference for Fy<sup>a</sup> is less than twice that of  $A$  but, for the simplest case of  $\mu_i = 0.5$ , Fy<sup>a</sup> is about sixteen times more informative (comparing likelihood ratios) than  $A$  with regard to population origin. The Fy<sup>a</sup> gene is, in fact, probably the best single gene discriminator now available for these two populations<sup>1</sup>. This great superiority of Fy<sup>a</sup> over  $A$  for individual admixture estimates in American blacks parallels a similar finding (Fy<sup>a</sup> gives twenty-two times more information than  $A$ ) for group admixture estimates<sup>1</sup>.

These calculations emphasize how far short of ideal most genetic markers are for making racial distinctions. The minimum requirement of eighteen ideal loci for the majority of American blacks (those with  $\mu_i$  greater than 0.1) means that, contrary to the suggestions of some authors<sup>2,3</sup>, accurate individual estimates of admixture, using genetic markers, are not now possible for this or any other human hybrid population, although for some purposes, such as study of hypertension in a hybrid population where ancestry may be important<sup>2</sup>, estimates of individual admixture may be useful for reducing unexplained variation, even though they are not individually accurate.

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## Model for Evolutionary Origin of Chromosome Bands

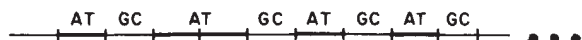
MANY eukaryotic chromosomes stained with quinacrine or by one of the Giemsa banding techniques show numerous bands in the chromosome arms. These have been termed Q or G-bands<sup>1</sup>. Autoradiographic studies show they correlate well with late replicating heterochromatin<sup>2</sup> and a number of biochemical and immunological procedures indicate the DNA in these regions is relatively AT-rich<sup>3-7</sup>. This raises the following enigma: as each banded region has enough DNA to hold several thousand genes<sup>5</sup> how is it possible for a given region of the chromosome to accumulate enough AT-rich genes to lead to a discernible band? In view of the rather gross folding pattern of chromatin in mitotic chromosomes<sup>8</sup> only a markedly non-random distribution of AT-rich regions could produce banding.

Some people have suggested the answer to be that the AT-rich banded regions are composed of repetitious sequences<sup>5,9</sup>. While it is true that the C-band regions<sup>1,10</sup> are usually enriched in repetitious satellite DNA, several observations suggest that this is not true for the intercalary heterochromatin in the Q and G-bands.

First, the Chinese hamster possesses many C and G-bands but very little satellite DNA<sup>11</sup>; its early replicating DNA is no more repetitious than its late replicating DNA<sup>12</sup>, and its

isolated heterochromatic DNA is no more repetitious than its isolated euchromatic DNA<sup>11</sup>. Second, the moderately repetitious sequences seem to be distributed throughout the non-repetitious sequences<sup>13</sup>. Further evidence for this comes from electron microscopy of moderately repetitious DNA reannealed to non-repetitious DNA which shows no evidence for clustering of the moderately repetitious sequences in *Drosophila*<sup>14</sup> or rats (personal communication from D. Holmes and M. Wilkes). Third, *in situ* hybridization has not yet provided any evidence for the clustering of repetitious sequences in the Q or G-band regions.

If the non-random distribution of AT-rich DNA is not due to repetitious sequences, how can it be explained? The answer may lie in the recent observation by Seabright<sup>15</sup> that X-ray-induced breaks tend to occur predominantly in inter-band regions. This suggests that there is some characteristic of the AT-rich intercalary heterochromatin<sup>7,8,16</sup> which protects it from radiation-induced (and probably sporadic) breakage. If confirmed, this single characteristic would be sufficient to account for the non-random accumulation of intercalary heterochromatin. Thus, a primitive organism may start with a stretch of DNA in which AT-rich inactive heterochromatic regions and relatively GC-rich active genes are evenly dispersed.



If breakage occurs in the GC-rich regions the following types of breaks will occur.



Subsequently, the following types of reunions will occur:

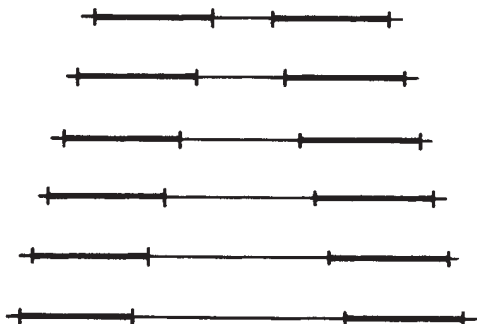
(a) those with fusion of heterochromatic regions,



(b) those with almost fused heterochromatin,



and (c) those with fused euchromatin.



Because of the decreased breakability in heterochromatin, types a and b would tend to persist, while type c would continue to undergo further breaks. This would eventually lead to the non-random accumulation of AT-rich heterochromatin and relatively GC-rich euchromatin sufficient to result in detectable banded regions. Presumably the separation would be far from perfect and the euchromatin would contain numerous small segments of AT-rich heterochromatin,



and the heterochromatin would contain numerous regions of relatively GC-rich euchromatin.

This would be consistent with the stability of the banded regions<sup>17,18</sup> and the observation that many more bands are present in chromosomes that are extensively despiralized<sup>19</sup>.

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## Nitrogen Fixation in Termites

SOME termites can live on a diet of cellulose filter paper<sup>1</sup>. This characteristic prompted Hungate<sup>2</sup> to question the possibility of N<sub>2</sub> fixation in termites by estimating the N balance of growing termite colonies, but he found no net increase in total colony N. Tóth<sup>3</sup>, on the other hand, incubated macerated termites in an N-free organic medium and observed an increase in fixed N in the solution. He used long incubation times, however, and possibly selected for N<sub>2</sub>-fixing organisms in the medium. We decided to reinvestigate the question of N<sub>2</sub> fixation in termites by exploiting the sensitivity of the acetylene reduction assay, a reliable indicator of N<sub>2</sub> fixation<sup>4,5</sup>. Preliminary reports of these results have already been published<sup>6,7</sup>.

Fifty workers of the subterranean termite, *Coptotermes formosanus* Shiraki, assayed immediately on removal from a nest of wood, were placed in a 10-ml vial containing 12 μmol C<sub>2</sub>H<sub>2</sub>. Ethylene, as measured by gas chromatography<sup>5</sup>, was produced at a rate of 0.122 nmol h<sup>-1</sup>. C<sub>2</sub>H<sub>4</sub> formation was dependent on the presence of C<sub>2</sub>H<sub>2</sub>, and was proportional to the number of termites used. All termites remained alive during the experiment, but the rate of C<sub>2</sub>H<sub>4</sub> formation decreased with time, perhaps owing to some toxicity of C<sub>2</sub>H<sub>2</sub> for the termites. Consequently, we routinely used a standard incubation of 1 h at 30° C to obtain initial reaction rates.

The hindgut of many termites is packed with cellulose-digesting protozoa, and bacteria such as spirochaetes, and is the site of considerable microbial activity<sup>8</sup>. It thus seemed likely that it was also the site of N<sub>2</sub> fixation. When the guts of intact termites were removed<sup>9</sup> and assayed, C<sub>2</sub>H<sub>2</sub>-reducing activity was associated with the guts. No activity was asso-



**Table 1** Effect of Diet on  $C_2H_2$ -reducing Activity of *C. formosanus*

| Diet                                            | Specific activity* | No. of bacteria per gut |
|-------------------------------------------------|--------------------|-------------------------|
| Wood (colony)                                   | 0.122 ± 0.048      | 2.6 × 10 <sup>6</sup>   |
| Additions to Whatman No. 2 filter paper †:      |                    |                         |
| No additions                                    | 1.310              | 3.4 × 10 <sup>6</sup>   |
| (NH <sub>4</sub> ) <sub>2</sub> SO <sub>4</sub> |                    |                         |
| 0.8 mg                                          | 0.810              | Not determined          |
| 8.0 mg                                          | 0.007              | Not determined          |
| KNO <sub>3</sub>                                |                    |                         |
| 1.2 mg                                          | 0.197              | Not determined          |
| 12.0 mg                                         | 0.007              | Not determined          |
| Antibiotics ‡                                   | 0.000              | 0.0 × 10 <sup>6</sup>   |

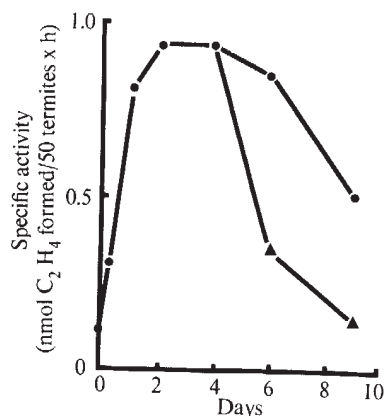
\*  $C_2H_4$  (nmol) formed from  $C_2H_2$  per 50 termites per h, determined after 10 d feeding.

† All filter disks were impregnated with 0.6 ml of a basal salts mixture containing (mg per 100 ml distilled water): KH<sub>2</sub>PO<sub>4</sub>, 30; MgSO<sub>4</sub>·7H<sub>2</sub>O, 20; MnSO<sub>4</sub>·H<sub>2</sub>O, 2; CaCl<sub>2</sub>·2H<sub>2</sub>O, 5; Na<sub>2</sub>MoO<sub>4</sub>·2H<sub>2</sub>O, 1 × 10<sup>-2</sup>; FeCl<sub>3</sub>, 0.2; ZnSO<sub>4</sub>·7H<sub>2</sub>O, 1 × 10<sup>-2</sup>; MnCl<sub>2</sub>·4H<sub>2</sub>O, 3 × 10<sup>-3</sup>; H<sub>3</sub>BO<sub>3</sub>, 3 × 10<sup>-2</sup>; CoCl<sub>2</sub>·6H<sub>2</sub>O, 2 × 10<sup>-2</sup>; CuCl<sub>2</sub>·2H<sub>2</sub>O, 1 × 10<sup>-3</sup>; and NiCl<sub>2</sub>·6H<sub>2</sub>O, 2 × 10<sup>-3</sup>. The pH of the solution was adjusted to 7.0 with NaOH. Termites were transferred to fresh filter disks after 7 d.

‡ Tetracycline, 480 µg; penicillin, 540 µg; chloramphenicol, 600 µg; and streptomycin sulphate, 600 µg.

ciated with de-gutted termite bodies. The  $C_2H_2$ -reducing activity of guts was only about 16% that of the intact termites, and undoubtedly reflected less-than-optimal assay conditions in the vial. These data suggested that microbes present in the alimentary tract were responsible for  $C_2H_2$  reduction in termites.

The  $C_2H_2$  reduction assay for N<sub>2</sub> fixation is based on the behaviour of  $C_2H_2$  as an analogue of N<sub>2</sub>, and its reduction to  $C_2H_4$  by nitrogenase<sup>4</sup>. Significant catalysis of  $C_2H_2$  to  $C_2H_4$  is known only in those biological systems capable of fixing atmospheric N<sub>2</sub>. Nevertheless, we sought further to establish that  $C_2H_4$  formation from  $C_2H_2$  by termites actually represented N<sub>2</sub> fixation. Direct demonstration of an enrichment of combined <sup>15</sup>N from an atmosphere enriched with <sup>15</sup>N<sub>2</sub> (ref. 10) was not possible because of the relatively low  $C_2H_2$ -reducing activity of termites. Nitrogenase synthesis (and from this  $C_2H_2$ -reducing activity) by all N<sub>2</sub>-fixing systems studied,



**Fig. 1** Modulation of  $C_2H_2$ -reducing activity of *C. formosanus* by dietary  $NH_4^+$ . Two groups of fifty workers each were fed Whatman No. 2 filter paper moistened with a basal salts mixture (see Table 1). On day 4 one group was transferred to fresh filter paper impregnated with basal salts plus 8.0 mg (NH<sub>4</sub>)<sub>2</sub>SO<sub>4</sub> (▲); the other group was transferred to fresh filter paper containing basal salts with no added (NH<sub>4</sub>)<sub>2</sub>SO<sub>4</sub> (●). Specific activities up to 4 d were averages of the two groups.

however, is known to be repressed when  $NH_4^+$  is included in the growth medium<sup>11</sup>, and this therefore affords an alternative test. We attempted to determine if the  $C_2H_2$ -reducing activity of termites would be decreased by feeding them a diet supplemented with  $NH_4^+$  or other N-containing compounds. Groups of 50 *C. formosanus* workers were placed in glass Petri plates each containing a 7-cm circle of Whatman No. 2 cellulose filter paper. Filters were impregnated with an aqueous trace salts solution with or without the inclusion of a N-source and the termites were incubated with the moist filters at 25° C for 10 d. Termites readily ate the filters, and similar amounts of paper were consumed by each group. The termites were then assayed for  $C_2H_2$ -reducing activity (Table 1). Termites fed filter paper containing no added N-source had more than ten times greater  $C_2H_2$ -reducing activity than termites fed wood. Presumably the filter disks contained less combined N than wood from the termite nest. Addition of  $NH_4^+$  or  $NO_3^-$  to filters diminished  $C_2H_2$ -reducing activity to an extent

**Table 2** N<sub>2</sub> ( $C_2H_2$ ) Fixation in Termites and Other Insects

| Termite                        | Caste                           | Diet                      | $C_2H_4$ nmol formed per h |                |
|--------------------------------|---------------------------------|---------------------------|----------------------------|----------------|
|                                |                                 |                           | Per 50 termites            | Per g termites |
| <i>Coptotermes formosanus</i>  | Worker                          | Wood (colony)             | 0.122                      | 0.695          |
|                                | Soldier                         | Wood (colony)             | 0.027                      | 0.139          |
| <i>Reticulitermes flavipes</i> | Worker                          | Wood (colony)             | 0.043                      | 0.204          |
|                                | Soldier                         | Wood (colony)             | 0.014                      | 0.073          |
| <i>Zootermopsis</i> sp.        | Reproductive nymphs and workers | Wood (colony)             | 0.517                      | 0.272          |
| <i>Cryptotermes brevis</i>     | Reproductive nymphs             | Moist filter paper (12 h) | 0.622                      | 1.705          |

The following insects formed no detectable  $C_2H_4$  from  $C_2H_2$ :

| Insect*                                      | Common name            | Sex†  | No. of insects per assay |
|----------------------------------------------|------------------------|-------|--------------------------|
| <i>Acyrtosiphon pisum</i>                    | Pea aphid              | ND    | 50                       |
| <i>Attagenus megatoma</i>                    | Black carpet beetle    | M, F  | 50                       |
| <i>Blattella germanica</i>                   | German cockroach       | ND    | 10                       |
| <i>Camponotus</i> sp.                        | Carpenter ant          | Mixed | 16                       |
| <i>Dermestes maculatus</i>                   | Hide beetle            | Mixed | 10                       |
| <i>Drosophila melanogaster</i>               | Fruit fly              | Mixed | 50                       |
| <i>Lasioderma serricorne</i>                 | Cigarette beetle       | Mixed | 48                       |
| <i>Mezium americanum</i> (adults and larvae) | Spider beetle          | ND    | 50                       |
| <i>Musca domestica</i>                       | House fly              | Mixed | 15                       |
| <i>Oncopeltus fasciatus</i>                  | Milkweed bug           | ND    | 30                       |
| <i>Periplaneta americana</i>                 | American cockroach     | ND    | 1                        |
| <i>Rhyzopertha dominica</i>                  | Lesser grain borer     | ND    | 50                       |
| <i>Stegobium paniceum</i>                    | Drugstore beetle       | ND    | 50                       |
| <i>Supella supellectilium</i>                | Brown-banded cockroach | ND    | 10                       |
| <i>Tenebrio molitor</i> (larvae)             | Yellow mealworm        | ND    | 2                        |
| <i>Tribolium confusum</i>                    | Confused flour beetle  | Mixed | 50                       |
| <i>Trogoderma inclusum</i>                   | Large cabinet beetle   | M, F  | 50                       |

\* Adult insects were assayed unless indicated otherwise.

† M = male; F = female; mixed = males and females together; ND = not determined.



Fig. 2 Modulation of  $C_2H_2$ -reducing activity of *C. formosanus* by dietary  $NH_4^+$ . Experiment performed in a manner similar to that of Fig. 1 except termites were begun on a diet of filter paper impregnated with basal salts plus 8.0 mg  $(NH_4)_2SO_4$  (●). Termites were transferred to fresh paper without added  $(NH_4)_2SO_4$  on day 2 (▲).

which varied with the amount of N-compound impregnated on the filter. Addition of high concentration of N-compound to filters diminished  $C_2H_2$ -reducing activity below that of termites fed wood. Similar depression of  $C_2H_2$ -reducing activity was affected by impregnating autoclaved *Escherichia coli* ( $10^9$  cells) or aspartic acid (120 mg) on filters used for feeding. These data indicate that the  $C_2H_2$ -reducing activity of termites is modulated by the amount of N contained in the diet, and offer strong evidence that  $C_2H_2$  reduction in termites is mediated by nitrogenase.

When termites were fed filter disks containing antibacterial antibiotics, all detectable  $C_2H_2$ -reducing activity was abolished. This was paralleled by a loss of detectable bacteria (but not protozoa) from the hindgut (Table 1). Protozoa eventually disappeared after 20 d or longer. These data are consistent with the idea that prokaryotes in the termite gut are responsible for  $N_2$  fixation, and suggest that intestinal bacteria also influence the vitality of the protozoa.

The increase in  $C_2H_2$ -reducing activity of termites fed filter paper probably represents depression of nitrogenase synthesis by gut bacteria rather than marked proliferation of N-fixing organisms possessing constant levels of enzyme. The relatively small increase in the total number of gut bacteria (31%) compared with the dramatic increase in nitrogenase activity (974%) (Table 1) is consistent with this interpretation. Whether the marked diminution of  $C_2H_2$ -reducing activity by  $NH_4^+$  represents elimination of  $N_2$ -fixing bacteria from the gut, or degradation of pre-existing nitrogenase, cannot be concluded at present. Microscopic examinations, however, revealed an abundance of bacteria and protozoa in guts of termites fed  $NH_4^+$ -impregnated paper for 10 d, suggesting that if  $N_2$ -fixers are eliminated, other bacteria proliferate.

We evaluated the efficiency of the  $N_2$ -fixing system in *C. formosanus* by periodically measuring their  $C_2H_2$ -reducing activity after changing their dietary N content (Fig. 1). The  $N_2$ -fixing activity in *C. formosanus* increases rapidly when the termites are fed filter paper, a diet presumably low in combined N. A significant increase in  $C_2H_2$ -reducing activity was observed after 5 h of feeding: maximum activity was attained after 2–4 d. When filter paper-fed termites were transferred to fresh filter disks containing  $NH_4^+$ ,  $C_2H_2$ -reducing activity diminished sharply compared with a control group of termites transferred to fresh disks with no added  $NH_4^+$ .  $C_2H_2$ -reducing

activity of filter paper-fed termites also decreased, although more slowly. The 9-d specific activity of the filter paper-fed termites was lower than that generally observed (see Table 1). The specific activity of  $C_2H_2$ -reduction in termites fed filter paper for 25 d decreased to 0.216. This was paralleled by an observable decrease in the termites' feeding rate, and undoubtedly reflected a response to the rather unnatural environmental conditions imposed on these subterranean termites.

In a reciprocal experiment, termites were fed a diet of filter paper +  $NH_4^+$  and then transferred to disks containing no added  $NH_4^+$  (Fig. 2). This resulted in a rapid increase in  $C_2H_2$ -reducing activity, whereas the  $NH_4^+$  control group maintained low levels of nitrogenase activity. These data indicate that nitrogenase activity in *C. formosanus* responds rapidly to changes in dietary N content. The rapidity of the response is probably limited primarily by the time required to significantly change the concentration of ingested N in the gut. In termites studied, food passage through the gut takes about a day<sup>8</sup>. On the basis of our studies of the time course modulation of  $C_2H_2$ -reducing activity therefore we consider the  $N_2$ -fixing system of *C. formosanus* to be efficient, and potentially able to supply the termite quickly with combined N should the dietary level suddenly drop.

To determine the distribution of  $N_2(C_2H_2)$  fixation in insects, we assayed different termites, as well as representatives of seventeen genera of insects available from laboratory cultures, for their  $C_2H_2$ -reducing ability. Of all insects tested, only termites reduced  $C_2H_2$  to  $C_2H_4$  (Table 2). All castes of termites tested reduced  $C_2H_2$  to  $C_2H_4$ , but soldiers possessed the lowest activity. This was not unexpected because soldiers do not feed on wood as do workers: instead, they periodically obtain stomodaeal and proctodaeal nutrients from colony mates<sup>12</sup>. The relatively high  $C_2H_2$ -reducing activity of *Cryptotermes brevis* Walker was probably due to their feeding on paper for 12 h before assay.

$N_2$ -fixing bacteria, or their metabolic products, may serve directly as a N-source for the termites. It is also possible, however, that the termites obtain all their N from ingested wood, and merely obtain growth factors from the  $N_2$ -fixing bacteria, or from protozoa which might require  $N_2$ -fixing bacteria.

Termites are of obvious economic concern. Worldwide termite damage and control costs are estimated at 1.2 billion dollars annually<sup>13</sup>. On the other hand, termites are important ecologically. In natural environments, they play a role in the turnover of complex carbohydrates by degrading dead trees and other cellulose-containing materials to more simple carbon compounds. They also increase soil aeration and drainage. In view of the ability of their microbial symbionts to fix atmospheric  $N_2$ , termites may also play a hitherto unrecognized role in soil fertility by replenishing combined N compounds in the soil. This may be particularly significant in tropical regions where termites abound.

We are now attempting to isolate the bacteria responsible for  $N_2$  fixation in termites, and to evaluate the relevance of symbiotic  $N_2$  fixation to the vitality of the insect. The latter may have significant economic implications. If, for example,  $N_2$  fixation is indispensable for termite vitality, suppressive measures may be directed toward destroying the integrity of the  $N_2$  fixation system in these insects.

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## Erratum

IN the article "Significance of Lead Isotope Composition in the Blood" by W. I. Manton (*Nature*, **244**, 165; 1973) the axes of the figures were wrongly labelled during preparation for the printers. The corrected figures are reprinted below.

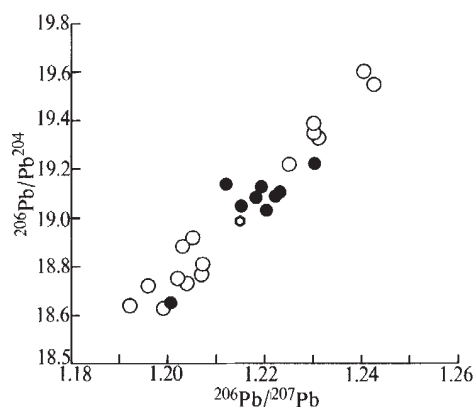


Fig. 1 Isotope ratios of lead in petrol collected in Dallas on July 14, 1972 (○), and of airborne particulate lead collected at various stations on July 18, 1972 (●). ○, Mean of the gasoline.

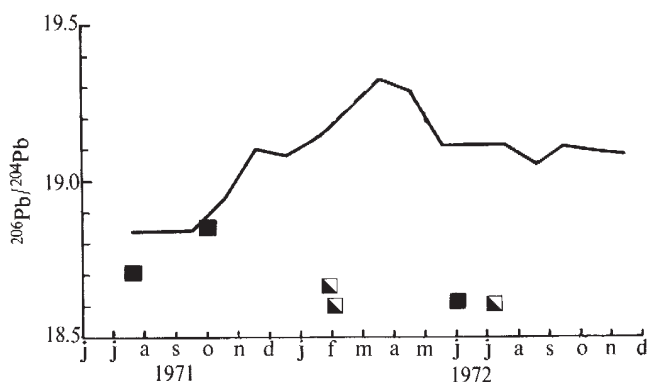


Fig. 2  $^{206}\text{Pb}/^{204}\text{Pb}$  ratio of airborne particulate lead over Dallas as measured from one station at monthly intervals (—). Also shown are  $^{206}\text{Pb}/^{204}\text{Pb}$  ratios from two smelters in the area (■, ■).

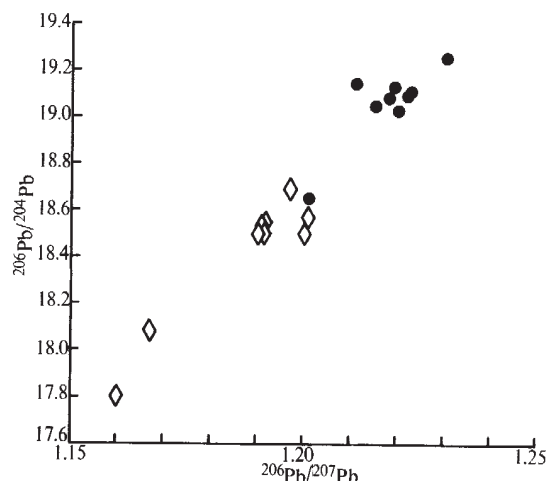


Fig. 3 Isotope ratios of airborne particulate lead over Dallas on July 18, 1972 (●), and blood of Dallas residents (◇) drawn in August 1972.

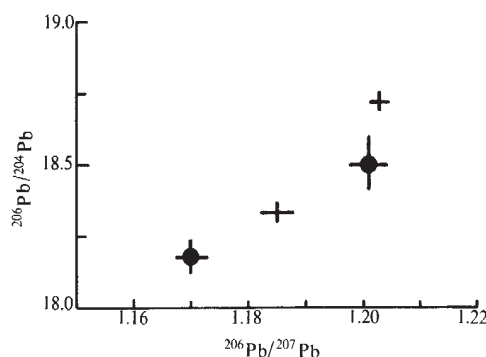


Fig. 4 Isotope ratios of blood of spouses W. M. and J. M. (+) and B. C. and T. C. (●). Bars represent 95% confidence limits.

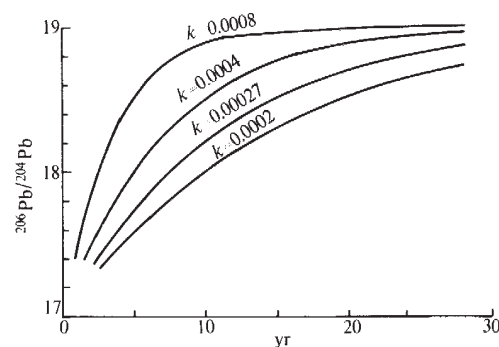


Fig. 5 Change in the  $^{206}\text{Pb}/^{204}\text{Pb}$  ratio of lead in blood from an initial value of 17 because of the absorption of lead with a  $^{206}\text{Pb}/^{204}\text{Pb}$  ratio of 19. Curves are shown for various values of  $k$ .

# BOOK REVIEWS

## Newtonian Algebra

*The Mathematical Papers of Isaac Newton.* Edited by D. T. Whiteside. Volume 5. 1683–1685. With the assistance of H. A. Hoskin and A. Prag. Pp. xxii+627+4 plates. (Cambridge University: London, July 1972.) £20; \$65.

THE present volume of Dr Whiteside's magnificent edition of Newton's mathematical papers is concerned with the history of the work that was later to be published as the *Arithmetica universalis*. In probably the first half of 1684 Newton, to comply with the statutes, deposited in the archives of Cambridge University a volume that purported to contain lectures on algebra delivered by him between 1673 and 1683. As Whiteside convincingly shows, the document can in no way represent a transcript of the lectures that Newton actually gave, and for at least one "lecture" Newton was not even in Cambridge at the stated time. The document in fact had been heavily rewritten and divided somewhat arbitrarily into separate lectures; Newton's intent is clearly shown where at one point he inadvertently addresses himself to the "reader". But it seems that the work actually had few readers before it was published in 1707 by William Whiston, Newton's successor in the Lucasian Chair. In 1722 a new edition was brought out which incorporated some revisions by Newton. In neither of these editions did Newton's name appear on the title page, although the authorship would have been obvious. The first edition was very favourably reviewed by Leibniz, but Whiteside finds that the work seems to have been little used until after Newton's death in 1727. It then had a certain vogue with further editions appearing, but was fairly soon to be superseded by other textbooks.

The main bulk of this volume comprises an edition and translation of the deposited "lectures", but Whiteside also includes various preliminary drafts and intended revisions. As with the previous volumes this one is greatly enriched by copious annotations, which give us the benefit of Whiteside's vast knowledge of seventeenth-century mathematical works, and he shows clearly how Newton incorporated various parts of his earlier work in this treatise—for example, notes that he had written for inclusion in the intended publication of Nicholas Mercator's Latin translation of a Dutch algebraic work by Gerard Kinckhuysen. (The translation and Newton's notes were published in Whiteside's volume 2.) My only regret is that the exigencies of producing the

volume could not have allowed a rather easier system of cross-referencing. In particular it is not always clear where information about changes made by Whiston for the 1707 edition or by Newton for the 1722 one will be found. Thus the reader who notices an apparent lack of correspondence between some of the problems in this edition and those in the 1722 edition may well miss the fact that this is discussed on page 184 note 216.

The treatise moves from elementary arithmetical operations to the algebraic treatment of quite complex problems. But from the beginning Newton treats operations on numbers in close conjunction with those on "species" or letters denoting unknown or indeterminately treated quantities. A central concern is the reduction of various problems to equations, which Newton explicitly regards as involving a translation from ordinary language to "algebraic language". Numerous problems are worked through as examples, and Newton pays particular attention to the use of algebra in the solution of geometrical problems. Considerable space is naturally also given to techniques for solving equations, and Newton enunciates various rules for identifying characteristics of roots of equations and the limits between which they will fall. The last section of the work as it stands (Newton did not regard it as complete) treats of finding first approximations for numerical solutions of equations by mechanical geometrical constructions. Here he reveals some doubts about the desirability of completely algebraicizing geometry, and maintains that "it is not the equation but the description that produces a geometrical curve" (page 424).

The nature of the material means that this volume brings to light less that is dramatically new about Newton than some of its companions. The meticulous scholarship that Whiteside has brought to it, however, helps to prepare for a fuller appreciation of Newton's algebraic work and reveals much concerning seventeenth-century approaches to this subject. A. G. MOLLAND

## Weisskopf on Physics

*Physics in the Twentieth Century: Selected Essays.* By Victor F. Weisskopf. Pp. xv+368. (MIT: Cambridge, Massachusetts and London, 1972.) \$7.95.

It was a good idea to collect in book form the most characteristic of

Weisskopf's essays on topical problems of physics and the social issues that its unprecedented expansion has raised: a source of renewed delight to his friends and pupils who already know its contents, this book provides to all enquiring minds and especially to the younger generation an introduction into the fundamental aspects of modern physics combining to a rare degree mastery of the subject with lucid simplicity of exposition. Indeed, the refined quality of these essays and the charm of their style would have deserved a less shabby production. The volume, introduced by a foreword (written by H. Bethe with warm feeling and deep understanding) and a delightful autobiography, is divided into four parts, respectively dealing with the basic conceptual framework of quantum theory, the main problems of the constitution of atomic nuclei and of the still more "elementary" constituents of matter, more special issues connected with the same topics, and finally some philosophical and social questions. On the latter the author discourses with his usual common sense, straightforwardness and generosity, but perhaps less pointedly than he used to do in our Copenhagen conversations with Placzek in the thirties. Although it is going too far to describe Weisskopf (as Bethe does) as "an American with European background", it is undeniable that the American environment has left its mark on his European outlook. There is one essay I sorely miss in this book: it is the one Weisskopf has never written about the epistemological breakthrough brought about by quantum theory—a theme he is so well armed to treat with persuasive brilliance. Confronted with the débâcle of epistemological thinking in the country of William James, why did he choose to shun the clamours of the Boeotians? L. ROSENFELD

## Nonscience

*Cults of Unreason.* By Christopher Evans. Pp. 264. (George G. Harrap: London, July 1973.) £3 net.

THERE can be few who read this book who have not at one time or another had at least a mild brush with one of the cults or organizations whose history Dr Evans tells; scientology, flying saucers, blackboxery (particularly of the De La Warr variety) and eastern mysticism from Gurdjieff to Hare Krishna. Dr Evans's thesis is straightforward and fairly familiar; the rationality of science



has diminished the credibility of traditional western religions, yet men in an age of uncertainty need some sort of a creed. The need is filled by a variety of new cults, many of which have attempted to come to terms with science by incorporating real, or more often spoof, scientific elements into their *Weltanschauung*. Having advanced the thesis, Dr Evans proceeds to illustrate it extensively from the history of Scientology and its strange creator, L. Ron Hubbard, from the Dianetics of the 1950s to today's incorporated "church of Scientology". He then deals more briefly with some of the other fringe cults, hopefully fitting them into the same thesis.

For the sceptic the book is fun to read; Chris Evans has worked hard to assemble a set of cheerfully anecdotal histories, which succeed in putting down in turn each of these more barmy manifestations of religion as the opium of the people with a healthy dose of Anglo-Saxon empiricism and a wicked eye for revealing detail. Believers, on the other hand, are scarcely likely to be convinced, if only because the author plays throughout the slightly unsatisfactory role of argumentation by story-telling. Clear analysis of just what the scientologists and the flying saucer fans believe and why is sometimes sacrificed for the throw-away punch line. Dr Evans could reasonably reply that he was not writing a "teach yourself Scientology" or another addition to the burgeoning literature of Ufology, but the blurb promises us more: not "a scientist's extended sneer at the intellectually inadequate . . . (but) the bases of the truce between what we know and what we don't know".

This is where I think Chris Evans has chickened out, for he has not done this. We never learn where he himself stands (except as an amused observer on the sidelines, watching the fans greet L. Ron as he arrives at Heathrow, or the shaven heads of the Hare Krishnas dancing down Oxford Street).

Clearly he does not "believe in" saucers, or Thetans, or the power of Yoga. But what, if anything, does he believe in—other than the validity of the scientific method? Without this perspective, as a scientific enquiry the book is like Archimedes without his fulcrum. As a sociological/philosophical enquiry, too, it lacks depth. Is it enough to assert man's need for some type of belief system without exploring more fully who needs what and why? Who becomes a scientologist? Is it true that there is a class difference between the Hare Krishnas (working class) and the Black Boxers and Gurdjieffians (upper middle)? It is a pity that the author does not address himself a little to these questions, even at the speculative level.

There is one area of conspicuous omission; the most substantive "cult

of unreason" today must surely be the conscious rejection of science, especially by the young, in search of an alternative mode of being à la Rozsak, a rejection of science undoubtedly occasioned by the very real oppressive role that it has come to play in the western world. No hint of this critique of science comes through Dr Evans's account, and in that sense it is curiously out of touch with a large element of his theme. Nonetheless, the book is both brave—for the wrath of the cultists when aroused is notorious, and their arm often disturbingly long—and fun, an entertaining history of some of the minor twentieth century lunacies. *Nature* readers should take it on holiday with them.

STEVEN ROSE

### Japonaiserie

*Proteins: Structure and Function*. Edited by M. Funatsu, K. Hiromi, K. Imahori, T. Murachi and K. Narita. Volume 1. Pp. x+381. £10. Volume 2. Pp. x+270. £7.50. (Kodansha: Tokyo; Halsted: New York and London, April 1973.)

ONE has become accustomed to publishers' *pots pourris* that contain collections of apparently unrelated items, linked only by the blurb writer's skill. What unites the articles in *Proteins, Structure and Function*—and this must be accounted an ingenious gimmick—is that all the authors are Japanese. The editors in their preface justify their selection by the suggestion that much of the Japanese work in protein chemistry is buried in domestic journals, sometimes in Japanese, and is therefore lost to colleagues working in the same field in the West. They have therefore encouraged their authors to give personal rather than archival views of their subject, so as to reflect the interest and style of their own laboratories, and consequently a rather large proportion of the references throughout are to papers by themselves and other Japanese authors. If this occasions a certain imbalance of treatment, it also makes for very readable accounts which specialists will for the most part welcome. The choice of subjects is necessarily arbitrary, for not all aspects of protein chemistry are represented in Japan. Volume 1 contains articles on enzymes of known sequence, trypsin, lysozyme, phage lysozymes and  $T_1$  ribonuclease, on which there are two separate chapters. The emphasis varies considerably from chapter to chapter in accordance with the known inclinations of the authors. Thus Inagami writing on trypsin places the emphasis on enzymic mechanism, Hamaguchi

and Hayashi strike a balance between enzymology and physical chemistry of egg lysozyme, the account by Tsugita and Ikeya-Ocada of phage lysozymes and  $\lambda$  endolysin contains a generous lacing of genetics, and the two chapters by Takahashi and by Oshima and Imahori on ribonuclease  $T_1$  concentrate on the physical chemistry of the enzyme and its interactions. Volume 2 has a rather curious assortment of subjects, directed, say the editors, towards mechanism and regulation of enzymes. There are chapters on amylases, bromelain, the toxic protein, ricin, threonine deaminases, ribulose-1,5-diphosphate carboxylases and the significance of N-terminal acetylation. The title has been cunningly chosen: it is not "*The Proteins*", and it obviously does not compete in any sense with the set of volumes edited by Neurath. "*Some Proteins*" might be an even better title, and it might well perhaps be regarded as a kind of supplement to the series of *Advances in Protein Chemistry*.

A. D'ALBIS

### Illustrated Taxonomy

*Classification of the Animal Kingdom: An Illustrated Guide*. By R. B. Freeman. Pp. 55. (English Universities: London, 1973.) £1.35.

THIS admirable book, by one of the country's leading taxonomists, is a fully illustrated classification of the animal kingdom which deserves a wide circulation among sixth formers, first year biology students at universities, and interested amateurs including every young nephew, niece and cousin who shows a taste for natural history. The classifications are authoritative, and the text is prefaced by an introduction on the principles and purposes of taxonomy. The illustrations (by the staff of *Readers Digest*) are beautifully executed and sometimes rather touching: thus the illustration of *Dugong dugon* (the Dugong) has "evolutionary dead end" written all over its rather morose features.

P. B. MEDAWAR

### Solar Environment

*Coronal Expansion and Solar Wind*. By A. J. Hundhausen. Pp. xii+238. (Springer: Berlin and New York, 1972.) 68 DM; \$21.60.

THE decade elapsed since E. N. Parker's monograph, *Interplanetary Dynamical Processes* (Wiley Interscience, New

York and London, 1963), has seen a flood of research into the solar-terrestrial environment. Accumulation of space observations, and of theoretical work stimulated by these data, has been increasingly demanding release through a new synthesis of the same ilk as Parker's. A. J. Hundhausen's new book satisfies this need in such a concise and lucid manner as to be equally suitable as an undergraduate text and as a research reference manual.

After a historical introduction, invaluable to non-specialists, Hundhausen classifies solar wind phenomena, identifying observed structures with characteristic theoretical time scales in the magnetized plasma. This provides a basis for the main themes of mathematical modelling of large scale phenomena in steady structureless flow of the wind and, subsequently, of non-steady flow in structured features.

Devotion of almost half the book to the steady flow problem itself indicates the "plethora of . . . partially proven theories" proposed to overcome the discrepancies between both one- and two-fluid wind models and observations. Though the author remarks, in typical style, that ". . . unique conclusions regarding the physical processes [which resolve this problem] . . . can be deduced only by true believers . . .", he succeeds admirably in laying foundations for future work by sifting the morass of free parameters and coronal boundary conditions variously adopted in models.

Following a chapter on the role of helium and heavier elements in the steady wind, structure is introduced into the flow and it is at once evident that high speed plasma streams superposed on the quiet wind are the rule rather than the exception. The apparent transience of such streams is emphasized as due to the confinement of rapid plasma flow within magnetic sectors co-rotating with the Sun and their theoretical description is developed in this framework. Correlation of these stream sectors with the large scale organization of the solar magnetic field, rather than with active regions, appears as one of the most important results of spacecraft observations.

Severe transient solar wind disturbances in the form of flare-driven shocks provide the last theme and exemplify the improvement of data in the last decade—for example, a reduction from 100 to about 2 in the uncertainty factor in blast-wave mass estimates. Seeking to find some small fault in such an excellent book on a wide topic, one might consider incompleteness or typographical errors. Near total absence of the latter and coverage of omissions in a final bibliographic chapter, however, allow only an unqualified recommendation of the monograph.

JOHN C. BROWN

## Nuclear Interactions

*Fundamental Interactions and the Nucleus.* By R. J. Blin-Stoyle. Pp. xv+345. (North-Holland: Amsterdam and London; American Elsevier: New York, 1973.) Dfl. 60; \$21.

FOR many years Professor Blin-Stoyle and his colleagues have made detailed studies of the manifestation of weak, electromagnetic and T-violating interactions in the nucleus. Now he has embodied the results of his studies in a comprehensive book that will be welcomed by all nuclear physicists.

After a short introductory chapter on fundamental interactions and the nucleus, the following four chapters (107 pages) are devoted to nuclear beta decay including a detailed consideration of the polar and axial vector currents, unusual hadron currents and interactions, and the lepton current and the neutrino. The next two chapters consider muon capture (27 pages) and the weak interaction potential (14 pages). The weak-interaction aspects of nuclear reactions follow, with chapters on the effect of a parity-violating potential in low energy processes, especially in alpha decay and in electromagnetic transitions (30 pages), and on time reversal invariance in nuclear reactions and in electromagnetic transitions (37 pages). Various electromagnetic and charge-dependent interactions are considered next (40 pages), with sections on the nuclear electromagnetic Hamiltonian, charge dependence of the inter-nucleon potential, magnetic dipole exchange effects and isospin multiplet energies, with a brief note on isospin in nuclear reactions. The last chapter is a very brief (8 pages) account of strong interactions in the nucleus. There are useful appendices on notation and conventions, relativistic and non-relativistic quantum mechanics, quantized fields, and isospin, charge symmetry and G-parity.

From this summary it will be clear that the scope of this book is much narrower than the title might indicate, and that it is concerned essentially with weak interactions and the nucleus, and even then there is no mention of electron scattering and muonic X-rays and the knowledge that can be obtained from them of the nuclear charge distribution. There is no account of the way the nuclear properties can be calculated from the nucleon-nucleon interaction or of kaon capture. The book is thus concerned essentially with the ways that various nuclear phenomena can be used to obtain more precise information about the fundamental weak nuclear interactions. As such, it is a very comprehensive and thorough review of a difficult field.

The need to make frequent use of field and group theoretical concepts and relativistic dynamics does not make for

easy reading, and although it is impossible to develop these *ab initio* some more help could have been given to the less well-prepared reader by references to standard textbooks and review articles. Apart from this, the theoretical background is clearly described, with derivations of the formulae needed to interpret the experimental data.

Many of the most important investigations in this field, such as the determination of the parity-violating potential in nuclear reactions, the charge dependence of the internucleon potential and the detection of T-invariant second class currents in beta decay, require extremely precise experiments and very careful evaluation of all possible correction terms. Such work is comprehensively described with detailed and up-to-date references to the original work, and clear tables showing the present status of the problems. For this alone it will be invaluable to all research workers in the field.

There is a full list of references at the end. It would have been helpful to include an index of author and a fuller subject index.

P. E. HODGSON

## Synthetic Method

*Advanced Organic Synthesis.* By Richard S. Monson. Pp. xii+203. (Academic: New York and London, June 1971.) \$15; £7.

WRITING books on chemical synthesis seems to have become as popular (though hardly as rewarding) an occupation as actually working at it, with several recent examples ranging from analysis of strategy, through systematic listings of functional group transformations, to descriptive accounts of the nuts and bolts of the subject. Dr Monson's book fits firmly into the latter category. It contains detailed procedures for the preparation of 250 and more organic compounds, the vast majority of them being uncomplicated, one-step operations. In this respect the book is very similar in style to *Organic Synthesis*, though it cannot match the scope and detail achieved by fifty-two volumes of the latter. In fact, many of the procedures are taken directly from *Organic Synthesis*, to which the author has added a few general introductory remarks. The experiments are chosen so as to exemplify the following processes: oxidation; hydride and related reductions; dissolving metal reductions; hydroboration; catalytic hydrogenation; introduction of halogen; miscellaneous elimination, substitution and addition reactions; the Diels-Alder reaction; enamines; enolates; the Wittig reaction; reactions of



trialkylboranes; carbenes; ethynylation; isomerization; carbon-carbon bond formation; and a few miscellaneous preparations including diazomethane and two peroxyacids; a few pages at the end are devoted to simple techniques such as distillation and adsorption chromatography.

This might seem to provide fairly wide coverage, with up to ten individual preparations under each heading, but there are a few serious omissions. The experiments chosen reveal a preoccupation with alicyclic chemistry; there are far fewer examples from acyclic chemistry, only a few involving aromatic chemistry and hardly any from heterocyclic chemistry; nor are there any involving amino acids. Several examples from these areas would have restored the balance which is lacking. A few more examples involving modern organometallic reagents are also called for. In a few instances the procedure recommended is neither the most successful nor the simplest available. For example, hydrolysis of the bromide on page 152 proceeds perfectly well in aqueous solution without adding silver nitrate; and the Wittig procedure on page 106 gives a poor yield of methylenecyclohexane. It seems not to be widely known that replacement of the solvent ether by diglyme in this experiment improves the yield to almost the theoretical value. Generally, however, the book fulfils its purpose, which is to provide an introduction to organic synthesis for beginning graduate students and advanced undergraduates.

M. A. MCKERVEY

## Day Length and Wobble

*Rotation of the Earth.* Edited by Paul Melchoir and Shigeru Yumi. With the cooperation of Lady Jeffreys. (International Astronomical Union/Union Astronomique Internationale. Symposium No. 48, held in Morioka, Japan, May 1971.) Pp. xxii+244. (D. Reidel: Dordrecht, 1973.) Dfl.40.

THIS book is based on papers given at the International Astronomical Symposium No. 48, on the "Rotation of the Earth", held in Morioka, Japan, in May 1971. Attending were geophysicists and astronomers from North America, Europe and Japan, as well as several scientists from the USSR. The book begins with a review by one of the editors, Dr P. Melchoir, outlining the developments in the field since the publication in 1960 of Walter Munk and Gordon Macdonald's classic by the same title.

Many of the papers were concerned with improvements in determining the

Earth's polar motion (wobble) and changes in the length of day ( $\Delta\text{LOD}$ ) by optical observations of the "fixed stars". Several new sources of information on wobble and  $\Delta\text{LOD}$  of the next decade, however, show potential for high accuracy. These include observations of near-Earth geodetic satellites, lunar laser ranging experiments and the use of very long baseline interferometry. In this respect, this volume provides invaluable references to the literature relating to these new developments.

There are also several papers discussing how internal motions of the Earth, such as core-mantle interactions and continental drift, can be measured directly using these new techniques of fundamental astronomy. Of special note is B. D. Tapley and B. E. Schultz's rigorous stochastic treatment of changes in the Earth's rotation, leading directly to models compatible with radar tracking data from near-Earth satellites. The reader with a more speculative turn of mind will find satisfaction in learning of recent developments relating earthquakes, solar activity and observation of irregular changes in polar motion. Earthquakes are also discussed as a source of excitation to the well known, but poorly understood, 14-month Chandler wobble.

The proceedings of this conference, when combined with L. Mansinha's collection of papers from a 1969 NATO Advanced Study Institute on *Earthquake Displacement Fields and the Rotation of the Earth* (D. Reidel, Holland, 1970), make valuable references on progress since 1960 for workers in fundamental astronomy and geodynamics, and an important one for earth science, astronomical, and basic physics libraries.

STEPHEN H. PLAGEMANN

## Radiomythology

*Low-level Radiation.* By Ernest J. Sternglass. Pp. xvi+166. (Earth Island: London, February 1973.) £2.50.

It must be presumed that Professor Sternglass never intended this book to be treated as a scientific work, otherwise he would not deliberately have misused scientific terms, used large numbers of very small units to give the impression of large quantities and been so highly selective in his use of the available data. Instead he has written a popular account, a story, of the ideas which lead him to conclude that the deleterious effects of low-level radiation from nuclear fall-out have been grossly under-

estimated, particularly in respect to infant and foetal deaths. Moreover, he claims that he was positively obstructed by the US Atomic Energy Agency and several leading scientists in presenting these ideas at the Congressional hearings on the effects of nuclear fall-out and in the scientific press.

One reason for his hostile reception can be found in his claim that "the nature of modern science depends upon the free communication of even the most disturbing ideas, since only through their widest possible examination can the essential process of the gradual correction of errors be accomplished". If there were no more to it than this a few imaginative people could swiftly put an end to all scientific progress. In this book Professor Sternglass has himself produced enough disturbing ideas to employ a whole army of scientists but he has not produced sufficient evidence to show that such effort would be rewarding. It is not good enough merely for him to provide the ideas that certain specific increases in fall-out might have been the cause of certain specific adverse changes in the health of selected populations. We need to know whether or not these ideas stand up to criticism when all the available data are considered, and, for example, if there is a relationship between foetal death and such diverse insults as a brief exposure to  $^{131}\text{I}$  and the slow release of  $^{90}\text{Sr}$  from maternal bone, what is the unifying factor and how it can be quantified in relation to risk.

It is now a respectable hypothesis that all releases of radioactivity to the environment can be expected in the long term to increase the incidence of malignant disease and genetic mutations throughout the world. All that is at issue is the magnitude of those increases and this is a matter which is under constant review by such reputable bodies as the United Nations Scientific Committee on the Effects of Atomic Radiation. By presenting his claims now without a reasoned appraisal of the views of such bodies Professor Sternglass can only further alienate the support of many scientists who honestly seek to find the true relationship between risk and low-level radiation. In this book he may have lost credibility even in the eyes of the most casual reader who, for example, will surely find it incredible that this man was actually waiting for the library doors to open in order to check that yet another trend in the US vital statistics supported his claims.

There is a real need for the relevant facts to be put in simple terms before the public. What a pity that Professor Sternglass, with his undoubted powers of popular expression, did not choose to present a more balanced view.

J. VENNART

## Microbes Used by Man

*Genetics of Industrial Microorganisms.* Edited by Z. Vaněk, Z. Hošálék and J. Cudlín. Volume 1: *Bacteria*. Pp. 496. Volume 2: *Actinomycetes and Fungi*. Pp. 511. (Elsevier: Amsterdam, London and New York, 1973.) Dfl. 225; \$70.25.

THE chemical activities of microbes for centuries have been exploited to produce commercially useful chemicals and microbial cell material. From the beginning, improvements and developments were pragmatic, which is not surprising in view of the lack of any understanding of the biochemical activities of living cells, and the genetic control of these activities, until around the turn of the present century. The realization that conceivably just a minute part of the corporate microbial genome had been exploited for industrial purposes came only in the late 1940s and early 1950s with the discovery of a range of commercially useful antibiotics, and later the use of microbes in production of valuable steroid drugs. With the accompanying explosion in the growth of information about the biochemical and genetic activities of microbes, even the less imaginative among industrial microbiologists began to believe that microbes might, by appropriate biochemical and genetical manipulation, be made to yield an almost limitless number of commercially valuable chemicals. These in many ways were the sentiments which prompted some 600 scientists to convene in Prague in August 1970 at the First International Symposium on the Genetics of Industrial Microorganisms. The keynote of the symposium was eloquently expressed in the title which Professor A. L. Demain gave to his address: "Marriage of Genetics and Industrial Microbiology—After a Long Engagement a Bright Future".

The proceedings of the symposium, in two volumes, have just been published—almost three years later. Such a long delay in publication must surely mean that at least some of the contributions are hopelessly out of date, especially since many of the participants to the symposium research in very active areas of molecular biology. The first volume deals with bacteria and the second with actinomycetes and fungi. Both volumes are well produced, which they should be with the swingeing price being charged for them. The first volume includes eight sections, dealing with genetics, mutagenesis, regulation of metabolism, and sporulation in bacteria. It also contains a most valuable section of four contributions on legal protection of strains, mutants, and genetic procedures, which includes accounts of problems which arise from litigation covering industrial microorganisms in Western (by W. L. Hayhurst) and

Eastern Europe (A. Stoy), the importance of accurately describing producing organisms (H. B. Woodruff and his colleagues), and the problems of novelty in relation to litigation in industrial microbiology (Z. Reháček). These contributions bring together information which is not readily accessible elsewhere, especially to the microbiologist, and is a most useful feature of the volume. The second volume of the proceedings is concerned principally with the genetics and metabolic regulation of streptomycetes and fungi, especially with the production of secondary metabolites such as antibiotics and alkaloids.

Nevertheless, the plain truth is that, over the past two decades, most of the major advances in industrial microbiology have still come from purely pragmatic approaches to problems of strain improvement, mainly through mutagenesis, and the refinement of cultural conditions. The contribution of genetics—the purposeful breeding of strains of industrial microorganisms with better performance characteristics—has been minimal indeed. This fact is reiterated, often with not a little coyness, in several of the contributions to these proceedings. Professor J. A. Roper, in an article on mitotic recombination and mitotic non-conformity in fungi, goes as far as to accuse the industrial microbiologist of a certain naïveté in believing that genetical manipulation could make a significant contribution to industrial microbiology. There have, undoubtedly, been technical barriers to the use of genetics in the microbiological industries, as well as biological barriers such as the possible existence of chromosome aberrations in highly mutated strains. Parasexual genetics has made some impact, especially in improving the quality of penicillin-producing fungi, but the contribution has been small, as Boyd Woodruff points out in his article in the second volume. Strangely, although the proceedings include a number of articles on metabolic control in yeast, there is no account of the role which genetical and biochemical research could make in improving brewing and baking strains of yeast. Here, surely, is an industrial microorganism which could be exploited genetically and biochemically; perhaps the brewers and the bakers are just not interested.

The contributions to these two volumes of proceedings contain a great deal of useful information on microbial genetics and biochemistry. Although they have been published far too late, these data are not to be found brought together elsewhere, and this in itself is sufficient recommendation to libraries to purchase the volumes. A second symposium on the Genetics of Industrial Microorganisms is to be held in Britain in 1974. Frankly, one cannot yet believe that the marriage to which

Professor Demain referred has been consummated. Even now, might it not be possible for the organizers of the 1974 symposium to call it "Genetics and Industrial Microorganisms" rather than "Genetics of Industrial Microorganisms"? ANTHONY H. ROSE

## Biological Control

*Principles of Biological Regulation: An Introduction to Feedback Systems.* By Richard W. Jones. Pp. xiii+359. (Academic: New York and London, March 1973.) \$16.

RICHARD JONES is no stranger to the problem of writing definitively about biological control systems. His earlier work in this field has largely been directed towards the reader with a firm grounding in engineering or physics. In this new book he has undertaken the much more difficult task of writing a book on the mathematical analysis of biological regulation, which is designed primarily for the life scientist.

The mathematical description of the principles of biological regulation is largely a question of organization. Historically, the understanding of homeostasis has progressed with each new insight into the organization of the organism from Bernard to Weiner and later workers. The continuation of this process inevitably leads to the use of mathematics.

The initial step in describing any biological system quantitatively is to organize the known facts in a block diagram or signal flow graph. In this book the author restricts himself primarily to the use of the block diagram with the simultaneous introduction of differential equations. The technique of developing each new mathematical idea in terms of a well known biological example is particularly useful, for it enables the reader, who is no doubt familiar with the qualitative description of the problem, to move more easily to a quantitative understanding.

Having introduced such concepts as transient response, and interacting and non-interacting processes, the author begins his discussion of feedback, which is the central principle of regulation. It is clear from block diagram algebra that any system which is to be analysed can be reduced to its canonical form, that is it can be reduced to one block in the forward pathway (efferent) and one block in the feedback pathway (afferent). In any discussion of regulation it is therefore logical to start from the canonical form.

This approach lends itself to the development of the open and closed loop gain equations and the concept of error. Within the book the under-



standing of these topics is rapidly reinforced by an appropriate examination of the operation of the thermoregulatory and blood pressure control systems.

The quantitative description of a biological process is likely to require mathematics a little more complex than the simple algebraic equations associated with the canonical form. This is particularly true when considering the dynamic response of a biological system. Behaviour of this type is often investigated by methods such as Bode and Nyquist analysis, both of which are clearly described in the text. However, immediately after this section of the text there is a brief introduction to the Fourier transform and its use in systems analysis. In an introductory work of this type a two-page summary on the Fourier transform would seem inadequate, or indeed even inappropriate.

Having described a repertoire of techniques, the author introduces the reader to their application in the analysis of human physiological systems. Such a discussion inevitably results in the consideration of more advanced topics, such as the analysis of nonlinear and oscillatory systems. These topics together with biochemical control form the material for the last two chapters. Clearly, these are areas of great scientific importance; hence they should either be discussed in detail or omitted completely. In view of the nature of this book, the latter course might be more appropriate.

In summary, then, the aim of Richard Jones to write a book on the quantitative description of biological systems is entirely laudable. In writing such a text the author is faced with the problem of steering a middle course between introducing mathematics which are beyond the scope of the reader but describe the biological situation accurately, or reducing the mathematical complexity to the point where the description is no longer realistic. Nevertheless, in spite of the reservations noted, the result is a well balanced text which is certainly worth reading.

R. I. KITNEY

## Population Ecology

*Populations in Seasonal Environment.* By S. D. Fretwell. Pp. xxiii+217. (Princeton University: Princeton, New Jersey, January 1973.) £5.75.

THE first part of the preface and section 2 of this book are excellent. The first part of the preface is a concise summary of the philosophy of science which is most welcome in a book of this sort. Section 2, which is based largely on the author's own work with birds, provides an elegant example of scientific method,

and would be worth reading for this alone. The author has a flair for deduction, so elaboration of hypothesis into prediction is prominent in his work, and most elegantly done. His explanations seem both original and profound as, for example, when his analysis of habitat selection in a territorial species suggests with simple inevitability that there must be selective advantage in the behaviour of both the winner and the loser in the territorial encounter. This and other of Dr Fretwell's conclusions in this part of the book certainly have the generality that he himself modestly wished for them.

Not so with section 1. The models are exclusively about populations in which there is no secular trend in density. On page 5 the equation  $d = \frac{b}{1+b}$  (which is fundamental to all the models) depends on the condition that  $N = N(1+b)(1-d)$ . And see page 3: "On the average the declines equal the gains . . . so the population shows only random (*sic*) changes in size." This statement embodies a philosophical error that has long prevailed among a certain school of ecologists but it is disappointing to find it in one so perspicacious in philosophy as Dr Fretwell. It is a philosophical error because it ignores time. It is palpably false on a geological time-scale on which any population may flower and most populations become extinct. It is palpably false on the historical time-scale: during the last 150 years in Australia many populations have flowered and many others have greatly declined. On the unrealistically short time-scale of the working life of an ecologist some populations seem to remain steady, but how can one test such a hypothesis without writing into the prediction some arbitrary short period of time, which makes the whole exercise trivial? Of course, Dr Fretwell is not interested in lack of secular trend *per se* but only as evidence for density dependence. But stability on a short time-scale is poor evidence for density dependence as the work of Den Boer and Reddingius has shown. If Dr Fretwell had considered their work he may not have missed the obvious third contingency in discussing summer *K* and winter *K*.

The logistic theory had a great vogue some five or six decades ago; but there were also some classical criticisms of it in the decades immediately following. It was wrong to revive the theory without refuting the criticism of it. To explain *Thrips imaginis* in terms of the logistic theory is nonsense because none of the assumptions is fulfilled.

Section 1 may not be worth reading because it is trivial but section 2 is a must for all population ecologists especially those interested in birds.

H. G. ANDREWARTHA

## Colour Constancy

*Surface Color Perception.* By Jacob Beck. Pp. xiv+206. (Cornell University: Ithaca and London, February 1973.) \$11.50; £5.20.

THIS book is concerned with the enormous gap that exists between the approach to colour by the physicist and by the artist. The physicist or the physiologist starts with what is clear to him—the nature of the light coming to the eye from various objects, and the way that nerve signals are generated and modified, as can be recorded from the eye and the brain. The artist or the psychologist starts from the other end with what is clear to him—perceptions of the colour and the lightness of the objects themselves.

The author evidently sees that the psychological complexity with which he has to deal is well beyond the reach of today's electrophysiology, and therefore ignores the known way that nerve signals are generated and processed and received in various parts of the brain. Instead, his treatment is to speculate upon the way that one aspect of perception may interact with others to account for what subjects say they see, in a very large variety of experimental situations.

The central problem is the famous and ancient question of colour constancy—how we manage to name the colour and lightness of objects around us "correctly" even when the colour and brightness of the illumination are changed so that the light entering the eye becomes quite different.

This is exceedingly hard to explain, especially since what is perceived is strongly influenced by what is expected. One kind of organization considered is expressed as follows: "Whether the stimulus is seen as a white in reduced illumination, or as a grey, depends upon whether the stimulus cues and an observer's attitude induce the perceptual system to assimilate the sensory signals to a schema of a white plus a correction for the reduced illumination, or a schema for a grey surface normally illuminated". This and alternative formulations may help psychologists to form some idea of what goes on in the mind to correct (or misrepresent) the primary sensation in a wide range of carefully examined conditions.

Indeed the importance and value of the book lie in the thorough way in which the very many aspects of surface colour are treated, the great number of classified experiments that are briefly described (with references), and the enumeration with contrast of interpretations advanced by various authorities.

The book is beautifully produced, and colour-plate 1 is a fascinating illustration of the variety seen in object colours.

W. A. H. RUSHTON

# CORRESPONDENCE

## Infant Precocity

SIR,—Leiderman *et al.* reported<sup>1</sup> that social factors made an important contribution to precocity in African infants. We have found that Arab infants in Lebanon are also precocious.

In a study of mental development throughout the course of rehabilitation of severely malnourished infants<sup>2</sup> we also tested a group of thirty healthy infants of the same low socio-economic class, matched for age and sex, all growing well by an Index of Thriving<sup>3</sup>. The average monthly income of the family was only about US \$90 with four or five children per family, most of the mothers illiterate and a few fathers with primary education. Each child was tested at 2-week intervals by a bilingual Arab female psychologist using the Griffiths Mental Development Scale. This test has five functions, locomotor, personal-social, hearing and speech, eye-hand coordination and performance, the expected United Kingdom score being 100. A total of seventeen infants were tested for the maximal number of eight consecutive sessions and a follow-up test about 4 months later. The mean values and standard deviations are shown in Table 1. It will be seen that without exception the mean value was over 100 and frequently well above this. The score tended to be highest for locomotor function and lowest for hearing and speech (the area most affected by malnutrition). Sixteen children from among the original thirty were tested by the Stanford Binet Intelligence Test nearly 4 yr later when they

were still growing well, as part of a study of the long-term effects of malnutrition on mental development<sup>4</sup>. The mean ( $\pm$ s.d.) IQ then was 98.7 ( $\pm$ 7.1) with no precocity.

Yours faithfully,

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<sup>1</sup> Leiderman, P. H., Babu, B., Kagia, J., Kraemer, H. C., and Leiderman, G. F., *Nature*, **242**, 247 (1973).

<sup>2</sup> Yaktin, U. S., and McLaren, D. S., *J. ment. Def. Res.*, **14**, 25 (1970).

<sup>3</sup> Kanawati, A. A., Haddad, N., and McLaren, D. S., *J. trop. Pediatr.*, **15**, 233 (1970).

<sup>4</sup> McLaren, D. S., Yaktin, U. S., Kanawati, A. A., Sabbagh, S., and Kadi, Z., *J. ment. Def. Res.* (in the press).

## Grassy but not Graminae

SIR,—I should like to correct a misconception which appeared in *Nature*, **243**, 342 (1973). In "Grass Roots at the Base of the Neogene", M. D. Brasier comments on the likely ecological effects of "marine grasses" when these first invaded reef communities. He says, "It follows that remarkable changes must have occurred after the initial colonization of the seafloor by the Gramineae". Unfortunately, although the name "sea-grass" is commonly used to refer to marine angiosperms in general, they are not true grasses (members of the family Graminae). They represent a variety of groups within the monocotyledons<sup>1</sup>. *Thalassia*, the one genus named is, for

example, a member of the family Hydrocharitaceae<sup>2</sup>. The name "sea-grass" comes from the generally linear or strap-shaped leaves of these marine flowering plants, which are superficially like the leaves of many grasses.

Yours faithfully,

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<sup>2</sup> Tomlinson, P. B., and Vargo, G. A., *Marine Sci. (Miami)*, **16**, 748 (1966).

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**Table 1** Mean ( $\pm$ s.d.) Values for Seventeen Lebanese Infants on Griffiths Mental Development Scale

| Test      | Age (weeks) | Mental age | Developmental quotient | Locomotor | Personal-social | Hearing and speech | Eye-hand coordination | Performance |
|-----------|-------------|------------|------------------------|-----------|-----------------|--------------------|-----------------------|-------------|
| 1st       | 26.7        | 29.5       | 109                    | 111       | 112             | 103                | 110                   | 108         |
| s.d.      | 11.3        | 12.0       | 8.4                    | 15.4      | 11.6            | 10.6               | 14.7                  | 9.9         |
| 2nd       | 28.0        | 33.5       | 112                    | 115       | 115             | 111                | 111                   | 107         |
| s.d.      | 11.5        | 14.1       | 10.6                   | 19.3      | 9.2             | 11.6               | 7.8                   | 15.9        |
| 3rd       | 30.3        | 34.3       | 117                    | 114       | 115             | 111                | 108                   | 107         |
| s.d.      | 11.7        | 13.5       | 6.9                    | 13.4      | 8.1             | 8.7                | 8.7                   | 13.5        |
| 4th       | 32.3        | 35.3       | 109                    | 114       | 112             | 107                | 109                   | 103         |
| s.d.      | 11.5        | 13.7       | 11.2                   | 14.4      | 13.8            | 10.1               | 13.9                  | 14.2        |
| 5th       | 34.2        | 38.8       | 112                    | 116       | 115             | 108                | 109                   | 110         |
| s.d.      | 11.6        | 12.6       | 7.0                    | 12.5      | 7.5             | 9.3                | 9.0                   | 11.2        |
| 6th       | 36.2        | 40.9       | 111                    | 117       | 114             | 104                | 105                   | 111         |
| s.d.      | 11.5        | 11.3       | 6.9                    | 13.9      | 9.5             | 9.8                | 9.0                   | 8.1         |
| 7th       | 38.2        | 43.2       | 113                    | 115       | 114             | 106                | 109                   | 112         |
| s.d.      | 11.5        | 11.1       | 8.1                    | 15.2      | 9.3             | 9.9                | 11.3                  | 8.2         |
| 8th       | 40.5        | 44.5       | 111                    | 115       | 113             | 105                | 110                   | 109         |
| s.d.      | 11.4        | 10.6       | 7.5                    | 11.8      | 6.5             | 8.0                | 10.7                  | 10.9        |
| Follow-up | 57.6        | 61.4       | 108                    | 116       | 109             | 102                | 110                   | 106         |
| s.d.      | 12.9        | 11.4       | 10.4                   | 10.4      | 8.4             | 10.4               | 10.9                  | 10.4        |



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